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ORIGINAL COMMUNICATIONS.

A singular case of complete closure of the Vagina, with subsequent conception, and safe delivery at the full period of utero-gestation. By ROBERT P. SIMMONS, M. D., St. Louis, Mo.

Some time towards the end of March, 1846, I was consulted by Mrs. E. B. W., a respectable married woman of this city, in reference to a mal-condition of the sexual organs, which she said rendered her unhappy in her domestic relations, and which she greatly desired to have remedied, should it be thought practicable. It not being convenient at the time to learn the history, or to enter upon an examination of the case, I promised to wait on her in a few days. Accordingly, on the second day of the following month, (April,) accompanied by my friend Dr. John Barnes, whom I invited to be present, I visited Mrs. W. for the purpose of making an examination of her condition. Before proceeding to do so, she detailed to us in substance the following history:

In the month of July, 1844, she was confined with her first and only child; had a laborious and very protracted labour, in which she suffered almost beyond endurance; the child was still-born and unusually large; at the time of its birth she was exhausted and unconscious of what had happened. In a very few days after her accouchement, acute phlegmonous inflammation invaded the soft parts, terminating in sloughing, with excessive and long-

continued suppuration; and it was not till the middle of October following, that she could begin to date her recovery. Some time during the succeeding month (November) she became aware for the first time of her peculiar and unhappy condition—a complete closure of the vagina, which she observed “has continued unchanged to the present time.”

Mrs. W. was attended in her accouchement by a medical gentleman of this city, who corroborates all she has said touching the severity of her labour, but, for some cause or other, he lost sight of the patient shortly afterwards, and can give no account of her subsequent and protracted illness.

Mrs. W. is a native of Ireland, aged 40 years, sanguineo-nervous temperament, possesses a good constitution, and never, as she informed us, failed to menstruate at her regular monthly periods, as well since as previous to her long confinement in 1844, except when she was pregnant, and at the period immediately preceding this examination, which she missed, and which ought to have been, as she said, about the middle of March, 1846.

Mrs. W. willingly submitted to an examination, with the hope that something could be done to remedy the defect. Accordingly she was placed on the bed, with the thighs elevated and the parts properly adjusted to the light. On proceeding to the examination, we could perceive nothing defective or unnatural in the labia externa, clitoris and fourchette, but on separating the labia to their fullest extent, we found the nymphæ almost entirely obliterated, and a dense fibrous structure presenting to our view, extending from the orifice of the urethra to the fourchette, and across from the base of one labium to that of the other, blocking up the mouth of the vagina completely, and resembling more than anything in appearance the palm of one's hand, when in an extended position. Dim tortuous lines of cicatrices were perceptible, and the whole surface of this structure presented to the touch a smooth, dense, elastic feel, but no opening or cavity whatever could be perceived. Impressed as we were, however, that an orifice or orifices did exist, from the circumstance of the menstrual discharge not being interrupted, we continued the examination for more than an hour with the greatest care and minuteness; used the smallest sized probes, applied them to every point of this structure and in every direction to its surface; also to the rugæ of mucous tissue lining the clitoris, fourchette and orifice of urethra, but we were unable to discover a single orifice or fissure communicating with the cavity of the vagina or uterus. The orifice of the urethra was of usual size, and occupied its natural situation—admitting the easy introduction of a medium sized catheter and its ready passage to the bladder. Our attention was then directed to the exploration of the urethral canal, believing

it possible that a fistulous orifice might be found near the opening of that tube, through which the menstrual fluid made its way from the uterus. Probes of various forms and sizes were used, but without obtaining the object of our pursuit; and ascertaining from the woman, that at no time during her menstrual flow did she perceive the discharges, either per urethram or per anum, even in the slightest degree tinged with that fluid, we declined the further prosecution of our inquiry.

Unwilling at that time, to give a conclusive opinion with regard to the propriety of an operation for the restoration of the vaginal tube, I deferred doing so from time to time, until the latter part of July following, when the husband of this woman called to inform me that his wife began to suspect herself pregnant. I supposed at once that her fears arose from the suppression of menstruation, which I was aware had not appeared since the middle of February, this period being about six weeks previous to my first examination. I advised the man to go home and say to his wife that she was mistaken, assuring him at the same time that such an event was a total impossibility; and he, poor man, having too long had sufficient proofs of his wife's inaptitudes in this way, readily acquiesced with me in opinion. I called to see Mrs. W. on the 8th of August following; perceived that she had increased in size since I saw her last, and was about as large as one at the middle period of gestation. She repeated to me that she had not menstruated since the middle of February, and said that "she felt something move within her belly." The first time, as near as she could recollect, was about the middle of July.

Previously immersing my hand in cold water, I applied it to the abdomen, causing her suddenly to change her position from side to side, but I could perceive no motion. I then questioned her more critically than I had done, with regard to her sensations and aptitudes on the embrace of her husband. She declared to me her total incompetency to afford her husband any reciprocity in sexual intercourse, and also assured me that at no time since her misfortune in 1844, did she ever experience any other sensation than that of disrelish on his embrace. She further stated that she had a kind and affectionate husband, and it was on account of his privation alone, that she was induced to make her condition known to me. "If I am in the family way, it is as much a mystery to me as it is to you, Doctor, but from the way I feel, there *surely must be something living within me*"

The case now became exceedingly interesting, and although I still doubted the possibility of pregnancy, I offered no further objections to her own diagnosis, based as it was on evidence so conclusive to her, and which, the issue of the case has proven,

was more certain than the lights by which I was governed in physiological science.

Mrs. W. requested me to keep her case in view, and if she were pregnant to attend her in her confinement; assuring me at the same time that she would look to no other source, except to her God, for help in her approaching tribulation! I continued to visit her occasionally, and as often found her increasing in size, and the abdomen presenting the feel and contour of one containing a growing foetus in utero.

Fully satisfied as I was with regard to the actual condition of this woman, as herein stated, yet I felt solicitous for additional testimony, should she turn out to be pregnant, and with that view I obtained the consent of Mrs. W. to allow another professional friend, Dr. Cornelius Campbell, to make an examination of her case. He accordingly did so, with me, on the 3d day of September last. On this occasion we made the same close and minute examination that was done by Dr. Barnes and myself on the 2d of April,—used the smallest sized probes of every shape and form,—applied them in every direction and to every point of the fibrous structure blocking up the vagina; also between the folds of mucous tissue lining the labia pudendi, &c. &c., but we were equally unsuccessful as I had been before, for no passage or orifice whatever could be found communicating with the vagina.

The catheter was readily introduced into the bladder, as on a former occasion, but we could find no track from the urethra to the vagina or to any other cavity. Neither was there any malformation of the anus or fistulous orifices about its margin. This was the last ocular examination I had of the case; and I must here express my extreme regret that I was not permitted to have a cast or drawing taken of the parts, by which a clearer and more accurate idea could have been conveyed of the appearance and condition I have endeavoured to describe.

On the 20th of October last I was requested to visit Mrs. W. as a patient. She had been very ill the night previous, and complained of distressing pains in the back and loins, rendering her unable to be easy in any position more than fifteen minutes at a time. She was as large as one very near the full period of gestation, and by placing my hand on the abdomen I felt, for the first time, the motion of the foetus. During the continuance of a pain I made an examination per os externum, and found the structure referred to presenting to the touch the same feel that it did on former examinations. There was no fulness or distension of the parts which the descent of a foetus and collected waters within the membrane would indicate; I was, however, fully convinced that the poor woman was pregnant, and pre-

suming, on her own calculations, that she had not arrived at her full period of gestation, I took from the arm ʒxxx. of blood, and gave Tinct. acetatis opii gutt. xxx. She was much relieved, and continued comparatively easy for several days, but the annoying pains returned, and continued with more or less severity up to the time of her accouchement.

On the 19th of November, at 6 o'clock, A. M., I was summoned to visit Mrs. W. in haste. On entering her apartment I found her in strong labour, and on making an examination per os externum, I discovered a fulness with great tension and pressing down of the smooth fibrous structure blocking up the vagina; and this condition of the parts increased on the return of every succeeding pain, till in the space of two hours I could perceive evident fluctuation of the waters within the membranes, and by forcible pressure with the fingers could feel a hard body low down in the pelvis, which I supposed was the head of the child.

Not feeling disposed to assume the entire responsibility of this extraordinary case, and besides desiring to afford the professional gentlemen who had previously examined the condition of this woman, Drs. Barnes and Campbell, an opportunity of witnessing the issue of the case, they were ordered to be sent for. In the mean time the parturient pains continuing with powerful force, the patient was placed on her back, with the thighs separated, and opening the labia-externa with the thumb and fingers of the left hand, I carefully divided the integuments with the scalpel to the extent of one inch, commencing the incision at the most prominent part of the tumour, and on a direct line from the orifice of the urethra to the perineum. The structure divided was about three-fourths of an inch in thickness, and of a firm fibrous texture, clearly indicating that the occlusion was not the result of simple adhesion of the sides of the vagina, but the new formation of tendinous tissue. Whether the membranes containing the liquor amnii had previously ruptured, or whether they were punctured with the point of the scalpel, it is impossible for me to know; at any rate, on passing through the structure with the knife, there was a free jet and flow of water for a moment, and also considerable hemorrhage, but this was not to an alarming extent, having its source principally in the great vascularity of the parts surrounding and closing the vagina.

I was now able to ascertain the condition of the os uteri—found it dilated to the size of a twenty-five cent piece, the margin thin and of a natural feel, with the head of the child presenting. By this time Dr. Campbell arrived, (Dr. Barnes not having received the notice,) when we dilated the incision previously made to the extent of two inches, by carefully dividing fibre by fibre;

with a well guarded probe-pointed bistoury, cutting both anteriorly and posteriorly. The pains continued with wonderful force, and the os uteri dilating, we soon were able to perceive the hairy scalp of the child, and ascertain more clearly its position, which was the posterior fontanelle to the symphysis pubis. The soft parts readily and safely dilated as the head advanced; and although the labour was both tedious and laborious, until the head emerged from the inferior strait of the pelvis, not an untoward event occurred, and the woman was safely delivered at five o'clock, P. M. same day, of a well formed and living female child, weighing 8 pounds. At this date (January 2d, 1847) both mother and child are living and doing well, and the poor woman feels doubly compensated for all her past afflictions, by the entire removal of her physical impotency, and the new source of moral happiness which has been lighted up on becoming a mother.

It may be proper to add, that great care was paid to the subsequent management of this case, and by means of a tent and other necessary applications, the newly formed vagina has been maintained. Suppuration has entirely ceased, and new mucous tissue partially formed on the internal sides of the divided structure.

The physiological questions involved in this case are—how did the menstrual fluid make its exit from the uterus, and by what law of the animal economy did conception take place? The woman's own statement, (her husband also concurring in the same facts) was, that since recovery from her long confinement in 1844, which was about the end of November of that year, her condition had been at no time different from what it was at my first and subsequent examinations; yet, notwithstanding, she menstruated regularly up to the middle of February, and at that time was as much unwell as usual.

She could give no account of it, except that the discharge issued from between the labia externa was small in quantity, continued about three days, and during these periods she never experienced any pain or difficulty whatever. Here I must express another regret—that in the investigation of this case, I never had an opportunity of observing the appearance of the parts during menstruation, which at the time I made my first examination it was my settled purpose to do at the next or some period of their flow, not having the most distant idea that pregnancy was the cause of their suppression.

Was there a vicarious secretion from the inner surface of the labia pudendi and contiguous parts, or did the menstrual fluid permeate through the fibrous structure blocking up the vagina?

I am inclined to adopt the latter opinion, although we were unable, on the minutest examination, to detect that condition of the surface which such a process would indicate.

In contemplating the curious phenomenon of conception in this case, I find on the very outset of the inquiry, the physiological views I had long entertained on the subject are subverted, and not feeling prepared to attempt an elucidation of that wonderful and mysterious function of organic life—conception—on any other principle than that of the actual presence of the semen masculinum within the cavity of the uterus, I will submit the rationale of this singular case to those who are deeper read in nature's laws, and whose province it is to demonstrate the occult mysteries in physiological science.

Saint Louis, Mo., January 2d, 1847.

Extraction of a large stone by the lateral operation. By R. HAYWOOD, M. D. of Tuscaloosa, Alabama, (communicated in a letter to Professor Pancoast.)

DEAR SIR:—I take pleasure in reporting to you another operation of Lithotomy which I have performed successfully. * * *

I was consulted by Mr. Armstead, of Tuscaloosa county, thirteen miles from this place, in a case of stone in his son, that had manifested unequivocal signs of its existence from birth. I sounded him and found a calculus of large size. I prepared him for an operation as soon as the condition of his case would permit, his health having suffered greatly from the presence of this foreign body in the bladder, until he was in his thirteenth year. As soon as the necessary preparation was made I performed the lateral operation, and extracted a stone weighing 1 oz. 5 dr. 16 grs.

I should have completed the whole operation in six minutes, had it not been that the shape of the stone was oblong, and I seized it first in its longitudinal diameter. I changed the forceps, and then seized it by its transverse diameter and extracted it.

In five weeks from the operation I sent the boy home, perfectly well. His health is now good, and he is daily improving in strength.

I send you enclosed an analysis of the stone and its weight, shape, size, appearance, &c., by Professor Brumby of the University of Alabama, (a gentleman highly distinguished in our state,) which you will be much gratified to examine.

University of Alabama, September 23d, 1846.

Dr. R. Haywood.

DEAR SIR:—I have made a careful chemical examination of the calculus, which you gave me for the purpose on the 21st inst.

It was the largest I ever saw ; weighed 653.70 grs., Troy, though three small fragments had been detached from it, while in your possession ; was oblong-oval in form, slightly flattened ; in colour yellowish-brown externally, white, with a tinge of yellow internally ; its surface presented very distinct tubercles, and was coated with small, white, shining crystals. It was solid, compact, brittle, and easily cut or scraped with a knife ; and when divided by a very fine saw, in the direction of the larger diagonal, its structure was beautifully displayed, showing a series of concentric coats, that diminished in hardness to the small, pulverulent, central nucleus.

Though only partially soluble in cold, it softened readily in hot water, and formed a white gelatinous mass, which subsided as the water cooled. In a solution, cold or hot, of pure potassa, it was insoluble ; but it dissolved readily in dilute nitric, sulphuric, hydrochloric, and acetic acids. From these acid solutions, it was readily precipitated by excess of pure potassa or soda with the evolution of a urinous, ammoniacal odour. A few drops of ammoniacal nitrate of silver being added to a neutral solution of it in nitric acid, the peculiar colour of phosphate of silver instantly appeared. A fragment heated to redness in a small platinum crucible first turned black, then became white, and was neither consumed nor fused ; and, even before the blow-pipe, the white residue was fused without difficulty. I was unable, by the most careful experiments, to detect the presence of uric acid, in any sensible quantity.

You will perceive, from these results, that it must belong to the species denominated by chemists *ammoniaco-magnesian phosphate* or *triple calculus*. This species is rarely pure, but generally contains a mixture of phosphate of lime. I did not deem it necessary to investigate this point, as its determination would not have any influence on your treatment of your patient.

Yours most respectfully,

R. T. BRUMBY.

Report of HENRY T. CHILD, M. D., Vaccine Physician for the Northern District of the Northern Liberties of Philadelphia.

To the Board of Commissioners of the Northern Liberties.

GENTLEMEN : — During the quarter ending on the first inst., I vaccinated eighty-one persons, twenty-nine of whom were males, and fifty two were females, which, together with four hundred and forty cases previously reported, makes a total of five hundred and twenty-one cases for the year 1846.

During the last three months I have found that a large number

of persons were insusceptible to the vaccine disease, when vaccinated under the most favourable circumstances, probably owing to the fact that we have no epidemic small pox influence in our midst.

In a former report, I remarked that previous to the existence of the epidemic, which was then prevailing, the failures among the first vaccinations were about ten per cent, but at that time they were not more than two per cent. After the disappearance of the epidemic the proportion of failures began to increase, and at present they are over thirty per cent, although the insertion of the virus, (known by its effect on others to be good,) has been repeated several times. I believe it is a fact that in those countries where small pox has never prevailed, vaccination can not be successfully performed, and I am inclined to the opinion that when there is no epidemic small pox existing, many persons will not receive the genuine vaccine disease; hence they will not be properly protected, and when exposed to the influence of small-pox, may be infected with varioloid or the disease in a modified form, and therefore I repeat my conviction as expressed in a former report, "that re-vaccination is necessary, during the existence of epidemic small pox, as a test of the protection of the system."

All of which is respectfully submitted.

HENRY T. CHILD.

No. 132 Green street, N. L., 1st mo. 4th, 1847.

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Lectures introductory to courses on the various departments of medicine are often "abstracts and brief chronicles of the time." The professor does not feel himself necessarily restricted to the subject which he has to illustrate during the course. Common custom permits him to digress, and occasionally it happens, that he leaves the domain of medicine and wanders into other departments of science. We have heard of an introductory lecture to a course of Institutes of Medicine on 'Hunting,' which might, however, be regarded hygienically; and we have ourselves listened to a learned but somewhat imaginative discourse on the Natural History of the 'Seventeen year Locust'—*Cicada septendecim*—the prolegomenon to a course of lectures on the Theory and Practice of Medicine.

The lectures before us are somewhat discursive. A few are strictly introductory to the departments taught by their respective authors. Others touch more or less directly on the agitating question of *medical reform*—as it is commonly designated; and as few but reformers have much zeal in the cause, most of the writers are on that side of the question. Moral courage is demanded of any one who feels satisfied with the things 'that be,' and declares so openly; and it is too much the habit with the over-zealous—and therefore, too often intemperate, hasty, and injudicious—to consider those who do not belong to the move-

ment party 'arrogant,' as if the charge did not apply rather to active agitators, who are dissatisfied with arrangements over which they have no direct agency—although such agency would doubtless be most grateful to them.

It has been often said, that the present is eminently an age of improvement; and with this sentiment we fully accord. The profession, within the last few years, has advanced at a more rapid pace than probably at any former period of the same duration in medical history. We believe that there never were in the ranks of the profession so many who have had the benefit of medical instruction in the schools; and we doubt whether there was, at any time, so great a proportion of readers; and whether in all respects the profession was as respectable. Improvements may be, and are, suggested; some of them feasible; but most of them not; and it must ever be borne in mind, that modifications of educational systems which have been wisely suggested, and sustained by long experience, ought not to be made hastily; and that every great change is an experiment, which in a government—or system of twenty-nine governments—like ours, may not be found as satisfactory in practice as it seems to be sound in theory.

Let the youth intended for the medical profession have—as we remarked in our last number—the intellectual and moral training that befits the well educated gentleman;—let boards of examiners see that the candidate for practice has the necessary qualifications, and there can be no well founded cause of complaint on the score of insufficiency of attainments. The standard of qualification must ultimately be determined by such examining boards; and if they do their duty, the wildest reformer can see no necessity for periodical meetings of the profession with the expressed intention of pointing out, and attempting to rectify, defects, some of which are doubtless real; but even if real, their repeated promulgation is calculated to do more harm to the profession in its connexion with the people than all the vagaries of the empiric, and all the follies of homœopathy and mesmerism, against which certain journalists appear to direct their shafts, whilst they pass over in silence the employment of secret agents by members of our own profession, who, by publishing the results of their experience with such agents,

become the open abettors of quackery, and scarcely more elevated in the moral scale than the quacks themselves. Reform—it appears to us—is as imperiously demanded on the part of many of those already in the profession, as of such as are preparing to enter within its pales. We know it will be said, that the imperfectly educated, the low in the social scale, are more apt to embrace those very practices, and such may be the fact; still truth must compel us to admit, that examples are occasionally given by those who occupy high places, and whose conduct becomes the more injurious in consequence. Instances of this kind have been recently sufficiently and lamentably notorious, and may suggest interesting reflections to the members of the Convention who are about to congregate in this city.

But:—to the lectures before us:—

That of Dr. Dugas professes to be a sketch of the improvements in medicine during the present century. As in many similar productions, there is in it, we think, an overstrained eulogy of the medical profession. In the latter part of the following quotation, too, the exception appears to us to be brought forward as the rule; for so far as our own observation has extended, the mass of aspirants for medical honors have been impelled to enter the profession solely by their desire to obtain through it an honorable mode of subsistence.

“In Europe, where the Church, the Bar, the Army and the Navy are the high roads to political as well as to social preferment, we find the members of these professions almost exclusively derived from the wealthy and aristocratic classes of society. In our country, those who are ambitious of political station usually select the Bar. The medical profession is, on the contrary, made up of those whom neither wealth, nobility nor political ambition, prompts to seek any other than scientific distinction—that which can neither be bought with gold nor acquired by hereditary transmission, but which must flow from personal merit alone. It cannot be conceded that many enter this profession as a means of earning a livelihood; but it is an innate love of knowledge, a desire to look into the mysteries of nature, which prompts them, unconsciously perhaps, to select this in preference to other pursuits in which less scientific research is necessary.” p. 6.

The progress of medical knowledge in all its departments, during the present century, is a field which might be tilled most productively; but the author before us has scarcely broken the surface; so that the announcement on the title-page is not satisfactorily fulfilled. Not much opportunity—it is true—is afforded for detail in a discourse of the kind, but many of the materials might, we think, have been better selected.

We do not know what amount of authority the lecturer has for the following broad assertion.

“Modern medicine has been peculiarly successful in facilitating the treatment of infantile diseases. At a time when the patient’s testimony was the only source of information, a correct knowledge of the diseases of those deprived of language or of intelligence, as children and idiots, was unattainable; whereas, with the aid of physical signs and the *method of exclusion*, by which all organs ascertained to be in a healthy state are excluded from farther observation, there are few, very few, cases in which any difficulty will be experienced in the formation of a correct diagnosis.” p. 14.

Or for the following.

“There is no disease in which the usefulness of our profession is more signally illustrated than that commonly called bilious fever, for whilst, if left to the unaided efforts of nature it will almost invariably terminate fatally, it now rarely if ever does so under early and skilful medication.” p. 15.

Or for the following.

“The Medical College of Georgia may justly claim the merit of having been the first to promulgate the great reformation in the treatment of paroxysmal fevers.” p. 16.

The lecture “on practical education in medicine” by Dr. Watson naturally dwells, but in no measured terms of eulogy, on the New York Hospital, which, if we were to judge from the Announcements and other advertisements of the Medical Department of the University of New York, affords to the student of medicine facilities not enjoyed elsewhere, and of which we would infer from the same sources he is not slow to avail himself. Under this impression, indeed, students have been induced—we had almost said *enticed*—to visit New York, when they have been surprised to find scarcely any in attendance. So late, indeed, as the winter of 1845-6—according to the New

York Journal of Medicine—not more than twenty of the students actually took the Hospital ticket ; and in the lecture before us Dr. Watson grievously deplores their scanty attendance.

“There were probably,” he says, “between six and seven hundred medical students in this city (New York) during the last winter. I was on duty in the second surgical department of this hospital during the greater part of that time, and strange to say, *it was a rare thing to see the face of a single student in any of my wards during the whole of my attendance.*”

With what propriety, then, can the array of cases of sickness treated in that institution be adduced by any Medical College as an incentive to the medical student for preferring New York to Philadelphia, when in the latter city there are not only opportunities for clinical instruction, but these opportunities are actually embraced by numerous students. During the last official year—we speak from authority—not fewer than *one hundred and forty-six* students took the ticket at the Pennsylvania Hospital ; and from Jan. 1846 to Jan. 1847—the University of Pennsylvania requiring their candidates for graduation to have attended one course of hospital instruction—not fewer than *two hundred and eighty-three*.

Dr. Watson properly urges the importance of clinical instruction, and who denies it? But it is an interesting question, whether it be to the welfare of the patient or to the advantage of the student that *masses* shall congregate daily in the wards, disturb the febrile and inflammatory, and examine individually, and in succession, the physical signs in thoracic and other diseases. There can be, from the philanthropist but one answer we apprehend to the question ; and hence the clinical teacher is compelled to select cases—as was long the custom at the Philadelphia Hospital and is still at the College clinics—for illustration in the amphitheatre,—the student having, at the same time, ample opportunity for witnessing the surgical and general management of the hospital.

So far, then, as facilities are afforded and embraced, there would seem to be no comparison between the advantages actually derived from clinical attendance by the students of Philadelphia and of New York.

The author of the lecture gives a brief history of clinical instruction in this country and elsewhere, into which we cannot

follow him. We have only space for one or two comments and rectifications. He is not quite as cosmopolitan as we could desire. Not only the hospital to which he is attached, but other institutions in the city of his residence, occupy, we think, too high a place in his thoughts. For example, he says:

"The cause of education in our well-appointed colleges—and *especially in the rival schools of this city*, (rivals be it ever hoped only in their ability for usefulness,) is every year becoming more and more elevated."

Quod est demonstrandum. We pause at least for the evidence.

Dr. Watson is a reformer—doubtless the advocate of a judicious reform—as we were wont to hear of the advocates of a judicious tariff. The following sentiment appertains to many as well as to himself.

"Gentlemen, the medical institution which first comes up to the reasonable demands of the profession in this respect, and, possessing the facilities, requires of its graduates practical acquirements equal to those at present enjoined upon the students of European schools, will find its interest in the measure." p. 8.

We do not believe in this. Such an institution may be able to exclaim with Francis the first, "*tout est perdu sauf notre honneur*," or it may have to submit to its benches being filled by a "select few." We have now in mind a distinguished literary institution, in which it was determined to "raise the standard," and to render its highest honours difficult of attainment. This was done; and whilst the graduates of other institutions were annually numerous, this distinguished school did not confer its highest degree at any time on more than half a dozen; and in some years, we believe, not a single candidate presented himself. It may be said, that they who succeeded would be more respected and successful than the alumni of other institutions where graduation was more easy; but we have had no evidence of this; on the contrary we do not know a single case in which marked advantage has accrued to the graduate for the toil which he had to undergo for this more elevated collegiate distinction. Let a department of higher mathematics be established in our colleges, and even if filled by a La Place or a Gauss, it might be attended by a few pupils; whilst the benches appropriated to lower mathematics under an ordinary teacher would

be filled to overflowing. A certain, or rather uncertain, amount of knowledge is admitted by all to be requisite; but the general feeling—erroneous we grant it to be—is that high literary and scientific distinction is not necessary, and may be injurious in the transaction of the business concerns of life; hence we not unfrequently see the learned and accomplished physician dismissed, and the lowest empiric consulted in his place. It is strange, but not less true, that an individual ordinarily—and even more than ordinarily—intelligent will consent to confide in the professional judgment of one whose opinions on any other matter he would condemn.

On the subject of "College Clinics," Dr. Watson dwells at some length, properly urging, that "to dispensary service, when confined to the management of such affections as legitimately belong to it, there is no direct objection;" but college dispensaries should never be held up as sufficient to supply the place of hospitals.

The college *cliniques*, he remarks, were first started in a neighbouring city, "where hospital privileges had been so much restricted as to be of little service to the winter students." In this he is altogether misinformed,—if he applies the remark to Philadelphia.

"As an expedient for supplying this deficiency, they were unquestionably acceptable on the same principle that half a loaf is better than no bread. They were at first called by their true name; but this attracting little notice was soon altered. The example thus set, and the title thus assumed, were copied by the schools of other places."

Dr. Watson does not here do justice to the city of his choice. We believe, that much of the credit that attaches to the college clinics belongs to it, and that the very name was applied to the "clinical" service at the University of New York before it was adopted here.

"The *clinique epidemic* of the colleges, as my friend Dr. Bell has happily expressed it," [we do not see wherein the happiness lies,] "was last winter at its height. The students, one and all, were affected by it, and the hospitals were forsaken."

Certainly not in Philadelphia. The observation may and doubtless does apply to New York, but not to this city.

The lecturer adds—

"It is to be presumed, ere this, the disease has materially

abated. Such I am told, is the fact in Philadelphia, where it first broke out. The University of Pennsylvania, and some of the other schools there have already abandoned their dispensaries, and are now supplying their second course students with hospital tickets at their own expense."

Some one has evidently been gulling the worthy author. There is not a word of real history in this quotation. There has been no abandonment of their clinics by other schools, and consequent supplying of tickets to the hospitals gratuitously. The clinic of the Jefferson Medical College was never in so flourishing a state, and so extensively useful. One other clinic has been added to the list—that of the Franklin College. The Pennsylvania College has never had a regular clinic, and gives—as it did last year—to second class students tickets to the Pennsylvania Hospital; but no such thing has been done by the University of Pennsylvania, which—if it has abandoned the *name* of college clinics, professes to teach—*mutato nomine*—"demonstrative medicine" on one day of the week, and "demonstrative surgery" on another! It would be difficult to find any where a sentence in which so much error—doubtless unintentional—is contained in so small a compass.

Our opinion of hospital attendance for *masses* has already been expressed, and as *masses* must be supplied in the best mode that is practicable, we do not hesitate to affirm, as our conviction, that the introduction of "College Clinics"—we use the term generally appropriated to them, and see no valid objection to it—is the most important single addition to medical educational resources that has been proposed in recent periods.

"Gentlemen," says Dr. Watson, "if I know my own mind, I have no disposition to draw invidious comparisons, or to speak of the relative advantages of rival cities or of rival institutions." p. 12.

Why then does he exclaim, after having made the erroneous statements referred to above:—"and if hospital attendance there [meaning in Philadelphia,] is beyond measure superior to their misnamed cliniques, [he had before shown that both were desirable, and had not instituted an "invidious comparison,"] what are we to think of the facilities for practical instruction enjoyed by students within the walls of the New York Hospital?"

We do not see the *sequitur*. If the hospital attendance in Philadelphia be better than the college clinics, what are the facilities for practical instruction enjoyed by students within the walls of the New York Hospital? Such appears to be the proposition, which we are not Cocker enough to solve.

We have no doubt of the value of the New York Hospital, and of the entire honesty of purpose of the author of the lecture before us; yet it must be admitted by the best friends of the Institution, that the picture of it given by him *savours* of hyperbole.

"You may," he observes, "in other countries find larger hospitals, but none presenting a greater variety of acute and important diseases; you may find in other hospitals abler teachers, but none so [?] willing as we have been to give you our time and services for nothing. You may find in some few other institutions greater opportunities for autopsic [a vile word] examinations; you may find in the cabinets of foreign societies, more valuable pathological collections; you may, in other cities, even find larger libraries than ours; but look for all these together in any other hospital either at home or abroad, and you will look for them in vain [?] *I say it without fear of contradiction, you will not find a single hospital to compare with this,—not one that contains within itself so many advantages for both theoretical and practical study as the New York Hospital.*" p. 19. Shame, then, on the profession of New York!—Shame, on the Professors of the two "rival schools," the course of education in which "is becoming every year more and more elevated!" Shame on the six or seven hundred medical students in the city during the last winter—that in the second surgical department, during the greater part of the winter, "it was a rare thing to see the face of a single student."

The peroration of the lecturer is judicious and wholly unobjectionable. It is well worthy of being retained in the recollection of the clinical student.

"During the coming winter, it will be well for every medical student here, but more particularly for such as are not prepared to remain in town after the close of the college session, to visit the hospital daily. Let nothing prevent you from attending at the hour set apart for the purpose.

Come not merely with your ears open, as students often do.

Come not to gaze about the wards with idle curiosity. But rather, come like philosophers, with all your faculties awake, for examining, comparing, judging, and thinking for yourselves; and working, too, for yourselves, at every opportunity.

Pick out your own cases, and study these, and note these for yourselves. Do not attempt to listen to all that is said with the view of remembering it:—but listen well to what is said of the cases on your own list, and see if it be true. Attempt not too much.

Do not fall into the peurile fondness of hunting after strange sights, rare diseases, and great cases. The dressing of an ulcer, the setting of a bone, the treatment of a pleurisy, pneumonia, or fever,—these, and such as these, should be your study here. Striking cases you may look at; but the business that should fix your attention, is that which best prepares you for the daily business of the profession.

Finally, avoid that morbid appetite for surgical operations, so long magnified and so much over-rated. Surgery is a good thing, a useful, an excellent thing in its way; but too much of it is a great evil. And the sooner you find out this for yourselves, the better for your patients. p. 19.

Dr. Bullitt's lecture is his maiden effort at the Saint Louis University. It contains much that is interesting and generally well expressed. "My primary allegiance," he remarks, "is to the general cause of medical science, and then to the cause of the medical department of the Saint Louis University;" and he expresses to his class a hope, that he may be able so to discharge the duties of teacher in the institution, as to make his "labours tributary to the common good of our common calling." Such we have little doubt will be the case.

We had marked one or two passages for extract and comment, but we cannot find space for them. This same circumstance will prevent us from giving more than a passing notice of the other introductions before us.

The lecture of Dr. Harrison inquires into the obligations of the medical profession to society, and the obligations of the public to the medical profession; and is an interesting discourse.

That of Dr. Patterson indicates a cultivated intellect. The style is vigorous, tinctured somewhat with the German, and perhaps with Carlylism, which we abhor; but it is never mystical, and always sententious.

The lectures of Dr. Huston and Dr. Meigs were delivered before the class of Jefferson Medical College—the largest, we may add, that has ever attended medical lectures in this country; and therefore sufficient to inspire the learned professors with unwonted energy. Dr. Huston's discourse is a scorching and able disclosure of Hahnemannism, clearly expressed—and it ought to convince all, who are susceptible of conviction, that the whole infinitesimal system is founded on folly, and too often practised in imposture.

The discourse of Dr. Meigs is full of the fire and enthusiasm which have ever characterized him. "I acknowledge" he says, "that I am an enthusiastic admirer of my profession. My speech shows it, and my whole past life is a perpetual proof of it. But I love that profession as a ministry, not as a trade."

The following observations on what has been sarcastically termed "book learning," or "book knowledge," from one who is so eminent in the practical exercise of his calling, are a satisfactory answer to those ignoramuses who pretend to despise books.

"Gentlemen, no man can study medicine by himself. He must have help and that help comes from books. I never could have learned why I ought to do thus and so, to keep a lady patient from bleeding to death, or from perishing with convulsions, out of my own primary independent observation and reflection. If I have done so, I thank the fathers, as I thank every good man—whether Greek or Roman, Arabian or European—who in ages past has put upon record the things that have been observed in medicine, and if I know those things I owe that knowledge to them; I could never have learned it of myself. Therefore, I love books. I think there is no good physician without their aid—nor can be none. Instead of disparaging books and authors, I would rather glorify and honour them when meritorious." p. 8.

The lectures of Professors Bedford and Gilbert are strictly introductory to their courses. We would remind the former, who says, that "Doctor Robert Lee, of London, has recently made some valuable contributions on the subject of the nerves of the uterus,"—that the nerves depicted by Dr. Lee have, by a still more recent observer—Mr. Beck—whose memoir has received

a medal from the Royal Society of London—been affirmed to be no nerves at all.

From a remark by Dr. Gilbert, Dr. Watson will find, that the Professor of Surgery in the Pennsylvania Medical College has no objection to surgical cases being brought before a class, which is all that is done in the regular "school clinics."

"Without pretending for a moment," he says, "to present, in addition to the regular course, an *adequate surgical clinic*, whenever the character of the case, however, and other circumstances are such as to render it compatible with the feelings and general advantage of the patient, I will present to the class as much practical surgery as can, with propriety, be brought before them. The experience of the two last sessions warrants me in saying to you, that cases of this kind may be expected; which, with the ample surgical and medical clinics of that time-honored institution, the *Pennsylvania Hospital*, will afford you clinical facilities *equal and we believe superior to those of any school in the country.*" [!!]

Lastly. It is not many years since the excellent institution—the Baltimore College of Dental Surgery—which has done, and is still doing, so much to elevate the professional character of the dentist—was chartered by the State of Maryland; and already many interesting and useful productions have emanated from it. The lecture of Dr. Westcott may be esteemed one of these. It canvasses whether "dental colleges possess peculiar advantages over any other means of securing a dental education"—determining the question—as might be expected—in the affirmative, and we are not disposed to cavil at the decision.

We are pleased, to observe, that extensive, general, and professional attainments are urged as indispensable to make up the character of an accomplished dentist. Whilst manual dexterity is important, it is wisely maintained, that the operator should be a well-educated gentleman, and that a knowledge of every branch of medical science should enter into the list of his evidences of fitness to do credit to himself, to the avocation which he has embraced, and to the community.

A Practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus: with remarks on the value of Leucorrhœa and Prolapsus Uteri as symptoms of Uterine Disease. By JAMES HENRY BENNET, M. D., etc. etc, 12mo. pp. 146. Lea & Blanchard. Philadelphia, 1847.

The basis of the present work was submitted by the author, a few years since, to the Faculty of Medicine of Paris as a thesis, on his graduating in that University. Subsequently, more elaborated, the essay was published, in parts, in the London Lancet; and afterwards in a yet more extended form, as a separate publication, of which the present is a reprint.

From the brief autobiography given in the Preface, we learn that the author spent seven years in the Saint Louis, La Pitié, and Salpêtrière Hospitals of Paris, first as pupil, and then as "interne" or resident physician.

"Under such circumstances," says the author, "I cannot, certainly, be reproached with not having matured my opinions. In the first instance, they were formed after I had long enjoyed very great opportunities of seeing uterine disease. They have since been considered over and over again, and have stood the test of several years additional experience."

"Some of the views which I bring forward will, I believe, be found original,—at least, if I can trust the results of my bibliographical researches. I have also many details of great interest and importance to present, with reference to the various modes of *treatment* in inflammation, ulceration, and induration of the uterine neck adopted by the Paris physicians and surgeons—details which will, I believe, be new to most of my readers. Having carefully watched, during a great length of time, the effects of the treatment followed by the eminent Parisian practitioners, with whom the knowledge of this form of disease recently originated, and that under the most favourable circumstances—as their pupil or assistant—I have been able, I hope, to form a correct estimate of the comparative value of the different agents they employ. I have thus, I am also inclined to think, learnt how to avoid the exclusiveness which most of them show in the choice of their therapeutic agents."

One can scarcely repress a smile at the confidence with which many of the young members of our profession express their opinions, after a residence of a few years, or as it some-

times happens, of a few months, in Paris, and of which the preceding extract is no very bad example. The natural enthusiasm of youth, operated upon by the example and habits common, to a great extent, in the French metropolis, are perhaps a sufficient apology for this self-complacency, until time chastens the one, and better example corrects the other.

We have looked through Dr. Bennet's *brochure* with some interest to find the "original" views which he deems so important without discovering them, although we have found much in it to approve, and we are inclined to think that his "bibliographical researches" have not been remarkably extensive, even among Parisian authors, to say nothing of British and German.

The subjects embraced in this treatise are of great importance, and we regret to say, generally too little understood by those entrusted with the treatment of such complaints; this, rather than any novelties contained in the book, or that we have to offer, claims from us more than a passing notice.

The author asserts that inflammation of the neck of the uterus, with its sequelæ, ulceration and induration, "is an exceedingly common affection"—and "is the principal cause, also, of several morbid states which are generally, if not always, studied independently of any such origin; as, for instance, prolapsus of the uterus and leucorrhœa."

That inflammation of the neck "is the principal cause" of prolapse of the uterus, is not very clear to our mind, but that it is often mistaken and treated for the latter affection, we know from abundant experience—no mistake indeed is more common.

"With reference to leucorrhœa," says the author, "indeed, I have ascertained, to my complete satisfaction—firstly, that, setting aside cancerous disease, in the very great majority of adult females who have been exposed to sexual intercourse, a confirmed leucorrhœal discharge, whatever may be its nature, is accompanied by inflammation of the neck of the uterus; secondly, that this inflammation seldom exists long without producing ulceration; and, thirdly, that ulceration is *always* accompanied by more or less engorgement (swelling, with or without induration) of the substance of the uterine neck."

We have no doubt of the correctness of these propositions; they are true as far as they go; but they do not go far enough,

as it regards leucorrhœa. That inflammation of the vagina, and often of the neck of the uterus, precedes leucorrhœa, no one will deny; and we are prepared to admit with Dr. Bennet that, after no long time, ulcerations of the cervix uteri frequently occur; but this does not account for *uterine leucorrhœa*,—in which the discharge comes from the *cavity* of the uterus.

The author makes what he considers “a fundamental and most important distinction between inflammations and ulcerations which occur in the uterine neck of females who *have never conceived*, and those which take place in the same region in females who *have conceived*—that is, who have either miscarried or borne children.” In the consideration of these diseases, he carries this division throughout the book; and in doing so, we cannot doubt that he magnifies unduly the influence of conception on the permanent condition of the parts. Those who bear children, and especially those who suffer frequent miscarriages, are undoubtedly most prone to inflammation and its consequences in the sexual organs, if we except prostitutes, with whom such complaints are almost universal, whether they have conceived or not. It is rare to meet with a woman has been long “on the town” who has not more or less disease of the neck of the uterus, although she may never have suffered from any of the forms of syphilis; nor is it rare to find individuals who have conceived, and even borne one or more children, in whom the condition of the neck of the uterus has returned in its physical condition very nearly to the virgin state—to a condition in no respect less healthy than before conception.

The following fact noticed by the author is one of great importance in practice, and often not appreciated by the practitioner.

“The size and length of the cervix uteri vary considerably in different females—a fact which must necessarily be taken into consideration if we wish to appreciate the existence or non-existence of engorgement, or morbid increased volume, of the organ. Indeed, these physiological (anatomical) variations are so great, that were we to allow ourselves to be guided by size alone, as appreciated by the toucher or speculum, we should, undoubtedly be often misled, and induced to suppose that disease existed when it did not. In reality, a very voluminous healthy cervix uteri is perfectly compatible with entire freedom, even from un-

easy sensation. The difference in length of that part of the cervix uteri which projects into the vaginal cavity, is evidently owing principally, to the vagina being implanted, as it were, at different heights on the cervix, so that in some females it is merely a few lines in length, whereas in others it is an inch and a half, or more. This physiological elongation of the cervix uteri may, it appears, be carried to such an extent, that its free extremity reaches the orifice of the vulva. Dr. Henning, in his essays on uterine diseases, lately published in 'The Lancet,' mentions several curious cases of the kind."

The following observations on the subject of hypertrophy of the cervix accords so fully with our own experience, that we cannot refrain from calling the attention of the profession to their importance.

"If the hypertrophy is considerable and general, the prolapsus of the cervix is constant, the irritation great; and ulceration and abundant leucorrhœa are nearly always present. If inconsiderable, or limited to one region of the cervix, the surface of the organ may be free from the disease, and the uterus may prolapse only after long standing, or walking or great fatigue of any kind. These are by no means uncommon states. Indeed, I have no hesitation in saying, that a very large proportion of the cases, both of slight and of severe uterine prolapsus, which are met with in practice, and for which pessaries are so improperly used, are the result of chronic hypertrophy of the uterine cervix."

We would even go further than the author, and say that a large number of the cases treated as prolapsus are either simple inflammation of the cervix or engorgement of the uterus, which result in ulceration or hypertrophy from neglect, and too often from the irritation produced by the improper use of pessaries and other means for the cure of simple prolapse proceeding from relaxation of the uterine supports.

The author witnessed the treatment of a great many cases of cancer of the uterus in the Parisian hospitals, and the following is the result of his observations.

"I have hitherto always found cancers of the cervix uteri, whether ulcerated or not, incurable; like cancer in other parts of the economy. In other words, I have never seen an evidently cancerous tumour or ulceration of the cervix, respecting the existence and identity of which there was not a shadow of doubt,

cured—that is, dissolved, removed, either by local applications or by general treatment.”

Of “localized cancer of the cervix,” treated by amputation, he witnessed no better results. “The cases in which it (amputation) has been performed successfully, are generally considered to have been merely cases of inflammatory induration of the cervix,” in which he thinks deep cauterization is a much safer remedy.

The Dog. By WILLIAM YOUATT, Edited, with additions, by E. J. LEWIS, M. D. Member of the Academy of Natural Sciences of Philadelphia; of the Philadelphia Medical Society; of the Parisian Medical Society, etc. 8 vo. pp. 403. Lea & Blanchard, Philadelphia, 1847.

To the lovers of the dog, (and who does not love that brave and devoted animal?) this is a most acceptable volume. Not only to the sportsman and the naturalist, but to the physician, and the housekeeper of every grade, the history of this faithful and sagacious animal,—his diseases and his habits,—is full of interest. “The dog, next to the human being, ranks highest in the scale of intelligence, and was evidently designed to be the companion and the friend of man.” “Man,” says Burns, “is the God of the dog; he knows no other; and see how he worships him. With what reverence he crouches at his feet—with what reverence he looks up to him—with what delight he fawns upon him, and with what cheerful alacrity he obeys him!” An animal that guards us while we sleep, enjoys our pastimes, and watches with almost parental care the tottering footsteps of our children, has strong claims on our benevolence, and it is the duty of those who exact his services to minister to his wants not less in sickness than in health. To do this effectually, they must study his constitution, his habits, his diseases and their remedies. To all such we commend Mr. Youatt’s book. The medical reader will be surprised to learn from its pages “how many ills that animal shares in common with the human race;” whilst the general reader will find its pages rich with anecdotes and varied information. No inconsiderable part of this has been supplied by the

editor, who has, indeed, performed his part with much care and ability.

The work is beautifully got up by the Philadelphia publishers—paper, typography, binding—every thing is in keeping. The engravings representing the varieties of the dog are by Gilbert & Gihon, and are well executed.

A System of Human Anatomy, General and Special. By ERASMUS WILSON, M. D. Lecturer on Anatomy, London. Third American, from the third London edition. Edited by PAUL B. GODDARD, A. M., M. D. Professor of Anatomy in the Franklin College of Philadelphia. With two hundred and thirty-three illustrations by Gilbert. 8vo. pp. 610. Lea & Blanchard: Philadelphia, 1847.

Since the publication of the former American edition, a third edition has been issued by the author in London, containing some additions and improvements. The present edition, we are assured by the editor, "is a careful and exact reprint of the English work, with the addition of such other illustrations as were deemed necessary to a more complete elucidation of the text." Thus it will be seen that the value of the present edition is materially enhanced over that of its predecessors, and in its present improved form offers to the student all the assistance that can reasonably be expected from such a work.

Second Annual Announcement of the Medical Institute of Cincinnati, Session of 1847.

The lecturers of this Association are Drs. Judkins, Woodward, Warder, Kendrick, Vattier, Mendenhall, Raymond, and Stuart; several of whom are advantageously known as contributors to the Medical Journals, and all, we have no doubt, well qualified for the duties they have assumed. The lectures are to commence on the first Monday in March, and continue four months, embracing all the subjects usually taught in summer schools.

We are glad to see our younger brethren in "the Queen City of the West," exerting themselves in increasing the facilities for

gaining instruction in their beautiful town. We have ever regarded such enterprises favourably, and can see no reason why the attempt should not succeed in a city of so large a population as Cincinnati, when made in the right spirit and sustained with becoming energy. From the impossibility of carrying on dissections, and of giving demonstrations in anatomy on the recent subject, during warm weather, a full and complete course on all the branches of medical education, such as is necessary to qualify a young man for graduation, cannot be expected, even if it were possible, which no one will pretend, for the student to undergo the fatigue, in hot weather, of following the requisite number of lectures each day to complete the whole in four or even six months but attendance on one or two lectures a day will hardly be too great a tax upon his health, while it will materially assist his comprehension of what he reads; and when the subjects taught are collateral, and not mere repetitions of the winter lectures, great advantage must accrue to the industrious learner.

RECORD OF MEDICAL SCIENCE.

Remarks on the use of Quinine in Intermittent and Remittent Fevers. By L. A. DUGAS, M. D., Professor in the Medical College of Georgia.—Having received during the past season a number of communications requesting my views on the use of quinine in the treatment of our autumnal fevers, I beg leave to reply to them through the medium of this Journal.

The fevers of this section of our country being almost exclusively paroxysmal, it may be well to premise, very briefly, my views of their pathology, by which it will be perceived that I regard them as essentially different from those *continued* fevers more commonly encountered in colder latitudes, and which have been denominated Typhoid, Typhus, Follicular enteritis, Dothenenteritis, Jail fever, Ship fever, &c.

Our paroxysmal fevers are either intermittent or remittent at their onset; but, if not arrested, the former may, more or less early, become remittent, or the remittent assume the intermittent type; thus showing them to be only different degrees of the same disease. They both present the same paroxysmal phenomena, that is to say,

have regular periods of recurrence or exacerbation, and of declension; they are both preceded and accompanied by a general disturbance, more or less marked, of all the functions of the system, but more especially of those usually termed nervous, as those of sensibility and muscular motion. Lassitude, pains in the limbs, back and head, loss of muscular strength, are premonitory and persistent in both. The activity of the circulation, however great, is not continuous as in the phlegmasiæ, but partakes of the same paroxysmal character as the other phenomena. Indeed it may be established as a maxim, that no inflammatory disease even assumes the paroxysmal character, inasmuch as all inflammations pursue an uninterrupted course, whether they terminate in resolution, suppuration or gangrene. Wherever inflammation is exposed to ocular observation, it is never seen suddenly to disappear and to return at stated intervals, or otherwise; but it runs a uniform course which cannot be suddenly modified by the efforts of nature nor by any agent with which we are acquainted. Pure inflammation of internal parts, as pneumonia, pleurisy, acute articular rheumatism, enteritis, &c., observes the same course; there is nothing paroxysmal in these diseases; the febrile action is not attended with daily or periodical exacerbations, but gradually progresses to a certain point, and then gradually declines with the subsidence of the inflammation. Periodicity or the paroxysmal peculiarity is characteristic of the neuroses properly so called, of diseases of the nervous system, which modify the functions of remote organs, and which may be dependent upon congestion, but certainly never upon inflammation. We know of no organ, whose inflammation could furnish us any rational explanation of the varied phenomena of intermittent or of remittent fever. Let us however look to the nervous system for the solution of the problem of these fevers, and all becomes perfectly plain. The languor, lassitude, general and local pains, tremor, modifications of the capillary as well as of the general circulation and of the secretions, and, above all, the abrupt transitions from a normal state to one of great perturbation, and from this again to comparative health, together with the periodical returns of the morbid manifestations—all indicate manifestly great disturbance of that pervading system whose condition is reflected in every part of the body—the nervous system. There is no other system whose impairment could by any possibility yield us the phenomena above related; still less is there any single organ that could by any modification of its condition, produce such general perturbation of the acts of the economy. Intermitents and remittents then are unquestionably the manifestations of deranged innervation; and if arrested sufficiently early will be attended with but little injury to any organ. A continuance of frequent repetition of this derangement, however, may more or less seriously implicate the parenchymatous and discerning structures, inducing inflammatory action, and may even terminate in fatal congestions.

With these views of the pathology of paroxysmal fevers, we are led naturally to the use of such remedies as are calculated to arrest

or to modify the perversion of innervation. Theory alone would indicate a resort to any agent known to blunt the nervous sensibilities, and thus to diminish their mobility or tendency to perturbation. Narcotics would present themselves in the first line. Every one knows that opium, morphine, camphor, alcoholic liquors, sulphuric ether, &c., are valuable remedies in intermittent fevers. Indeed a favorite prescription with me in such cases is a combination of 2 parts of sulphuric ether, 1 part of tincture of camphor and 1 part of tincture of opium, of which I give a tea-spoonful in a wine-glass of cold water two hours before the expected paroxysm, and half this quantity again at the expected hour of attack; the patient remaining in bed during the effect of the remedy. This rarely fails in uncomplicated cases of intermittent fever; if it does not completely succeed the first day, it will the next. I have frequently averted, or favorably modified even a paroxysm of *remittent* fever by the administration of a full dose of morphine ($\frac{1}{4}$ to $\frac{1}{2}$ gr.) half an hour before the expected exacerbation. But the efficacy of narcotics is not so fully borne out by experience as is that of quinine, an agent which as yet holds a position unique in the materia medica. The most striking peculiarity of quinine is its power to prevent the return of periodical affections, and this appears to me to be effected by blunting the susceptibilities of the nervous system. The senses whose acuteness of perception we can most easily observe, are manifestly blunted. Audition is very soon impaired, and so is vision, if the dose of the remedy be large. The effect of quinine on the heart, in our fevers at least, is unquestionably to diminish the force and frequency of its action, and if the quantity administered be large, a general relaxation, attended with a profuse cold sweat, will be produced, resembling and therefore mistaken by the inexperienced for a collapse of fatal tendency. Having tried it in cases of pure phlegmasia, in pneumonia and acute articular rheumatism, for example, without any aggravation of the febrile action, I cannot regard it as a stimulant.

There is, I believe, no difference of opinion in relation to the value of quinine in the treatment of intermittent fevers. I will, therefore, now confine my remarks to Remittent fevers, comprehending under this term, bilious, malignant, congestive, and country fevers. These are usually preceded by premonitions, which if properly attended to, would enable us to avert their developement with great ease. It is, however, exceedingly rare that medical aid is invoked thus early, and the physician is generally called in only during the first or second strong paroxysm; often much later. The paroxysm, when once fully developed, will usually run its course despite of any efforts we may use to check it. I therefore generally direct merely a foot-bath, and the free use of cold drinks, as water, lemonade, or soda water, until the period of remission. Should there be, however, such a determination to some vital organ as to threaten serious injury before the equilibrium of the circulation be restored by the subsidence of the exacerbation, I abstract blood with cups to the spine, sometimes (though rarely) deplete from the arm, and urge the use of revulsives,

as hot and stimulating pediluvia, and sinapisms to the spine, epigastrium, feet, &c.; if the head be congested, the affusion of cold water to it, continued until the pulse be depressed, and repeated as this reacts, is the most efficacious application I know of. Saline enemata, especially if the bowels are full, should not be omitted, as cathartics will very rarely act during the stage of excitement. If the congestion be attended with cold clammy skin, a small and feeble pulse, and prostration of the vital energies, I advise, in addition to the revulsives, large and repeated doses of the above-mentioned combination of ether, laudanum and camphor, until reaction takes place.

The exacerbation having subsided, our treatment should be directed to the prevention of its return, and my invariable rule is *never to permit the occurrence of another paroxysm after I see the patient*. But, it will be asked, can this rule be carried out? I answer that it can *in the great majority of cases*, and that in those in which we fail to accomplish all we desire, we yet so modify the state of things that success is almost certain on the day following. If we be fully impressed with the belief that the fever being once arrested the patient will rapidly return to health, the importance of the rule cannot fail to be appreciated; and that such is the fact will not for a moment be denied by any who have ever tried the practice we recommend. I repeat, that if all our efforts be directed to the prevention of another paroxysm—if we resolve never to allow a patient to have another exacerbation after we see him, the cure of remittent fevers will almost invariably be effected in a day or two.

In the accomplishment of our resolve, quinine must be regarded as the sheet anchor of our dependence, for although we may resort to other means, these can never be but of secondary value. Nor is it necessary in ordinary cases to use such large quantities of the quinine as are recommended by some. The quantity I use in one remission is usually from 15 to 20 grs., but I have sometimes given 30 or 40 grs.; never more. It is rare that less than 15 grs. will prevent the expected paroxysm. Whatever be the quantity we may estimate as necessary, this should be given in such a manner as to have the system fully under its influence an hour or two before the time of the previous exacerbation, and to continue its influence a couple of hours after this time. If the period of remission be eight hours, we may administer 2 grs. hourly—if it be five hours, we may give 3 grs. hourly—if three hours, 5 grains hourly—and if only one hour, we should give 20 grs. at once, and smaller doses subsequently, if necessary, to insure success. According to my observation the *number* of doses is a matter of but little moment—the *quantity* given in a remission is all important. This will depend upon the violence of the attack, the number of paroxysms that have occurred before we see the patient, and the kind of treatment to which he may have been previously subjected. As a general rule, the quantity should be increased as the period of remission is shortened, and in proportion to the number of paroxysms that have preceded its use. I am inclined to think also that it requires more quinine to prevent a paroxysm in

one who has been depleted or acted on by emetics and cathartics than in one who has previously been subjected to no medication. The *convalescence* is certainly more rapid when no debilitating process has been instituted, and health is almost immediately restored if the disease be arrested with quinine on the occurrence of the very first paroxysm. There is some choice in the mode of administration, for the sulphate of quinine will act more slowly if given in powder than in solution, and still more so in pills than in powder. Whenever, therefore, a prompt effect is necessary, the solution should always be preferred. If the stomach will not retain it, it may be thrown up the rectum with a little flax-seed tea or thin starch, in about the same dose as if given by the stomach. In this way it acts remarkably well, and, in the treatment of children, who evince great reluctance to its taste, this mode of administration is peculiarly happy.

But the query is often made: would you give the quinine in cases of remittent fever in which the head is evidently affected,—when there is intense cephalagia, or coma, or delirium? in cases in which the stomach seems implicated—the patient vomiting frequently and rejecting everything he takes? in cases in which the bowels are too loose, or very easily disturbed? in cases in which the liver is either torpid or secretes inordinately? in cases in which one paroxysm runs into the succeeding so completely as scarcely to leave any remission of consequence? I answer, unequivocally, yes—and that the stronger the tendency of the disease to localize itself, the more urgent is the necessity to arrest it; for this tendency will increase with every paroxysm, and cease as soon as their return be checked. Let us always bear in mind that the paroxysms are not *occasioned* by the affection of the head, stomach, bowels, or liver, but, on the contrary, that these are the *consequences* of a deranged innervation and of the paroxysmal condition, and our duty is plain. Let us not be alarmed by the bug-bear inflammation and vitiated secretions, nor be deterred from the use of quinine because some still believe it a stimulant, and our success will very soon eradicate every vestige of former prejudices on this subject. It was not without much difficulty that I succeeded a few years ago in persuading a planter, who had long been in the habit of looking on bilious fever as occasioned by the presence of vitiated or superabundant bile, and who consequently treated his negroes with emetics, cathartics and mercurials, that if he would use quinine at the outset, his hands would be in the field in a few days, instead of losing from ten to fifteen days whenever attacked by fever. And yet, after he had fully satisfied himself of the advantage of the proposed change of treatment, his first observation on meeting me was always—“what becomes of the bile? I am afraid that it is still in the system and will again do mischief?”

In order to illustrate some of the positions I have assumed, I will relate a few cases in which the remission was very light, and the tendency to localization imminent.

On the 12th of October, 1841, I was called to see a lad about 10 years of age, and found him in the height of the second paroxysm of

a most violent attack of remittent fever. The pulse was full, strong and active; the heat of the surface intense; he complained of violent head-ache, yet was incessantly tossing himself about the bed in wild delirium; his stomach and bowels were quiet. I had but a few days previous seen a patient about the same age, and in the same neighborhood, succumb (without quinine) in the third paroxysm of a similar attack, and I had every reason to apprehend a similar issue in this case, if another paroxysm were permitted to occur. It was now 2 o'clock, P. M. and the next paroxysm was expected to commence at 8 in the evening. He had taken a cathartic the day before I saw him. I immediately opened a vein, to prevent increased injury to the brain, and abstracted blood pretty freely; then applied a blistering plaster over the dorsal region of the spine, and commenced the use of quinine in doses of 2 grs. every hour. At my evening visit (7 o'clock,) I found him quiet, free from delirium, and with very little fever. Ordered the quinine in doses of 1 gr. hourly through the night. The next morning I found him sitting up without fever, and wishing something to eat. He had no return of fever, took no more medicine, and was perfectly well in a few days. I would remark that the delirium entirely subsided only after he had taken several doses of quinine. I have since given it during delirium, without bleeding, and with equally good effect.

On the 28th October, 1841, I was requested to visit a gentleman, about 45 years of age, on the 5th day of a severe remittent fever. I found him with high fever, lying on his back, and so comatose that it was with considerable difficulty that he could be made to notice questions, to which he would then make incoherent replies. His surface was moist with perspiration, though warm. His pulse was frequent, and somewhat strong, but not sufficiently so to warrant bleeding at so advanced a stage of the case and especially as he was of intemperate habits. He had taken two or three cathartics—and the onset of the next paroxysm was expected in three hours. The case was such, that death must of necessity attend the supervention of another paroxysm. Under these circumstances I ordered 5 grs. quinine in solution every hour, and remained to watch the effects, for I was not at that time as well acquainted with them as at present. Indeed I had not before ventured the use of quinine under a similar determination to the head. The administration of each dose was attended with manifest improvement, so that when the time arrived for the recurrence of the paroxysm, my patient was perfectly lucid, had no stupor, and but little fever. I then left him, with orders to take 1 gr. of the quinine hourly, for twelve hours. On the following morning he was sitting up, without fever, and had none afterwards. A mild laxative was all he took during the rapid convalescence.

During the same month, I attended a girl 8 years of age, whose remittent fever was marked by great gastric irritation, so as to cause her to reject every thing she took; quinine solution administered *per rectum* as readily controlled the disease in this as it did in the above cases.

More recently, I saw a gentleman who had been seized at 9 o'clock A. M. with a chill, which was soon followed by the most intense head-ache, intolerance of light, pain in the back and limbs, as well as at the epigastrium. Being of a sanguineous and plethoric habit, I bled him; then applied sinapisms to the spine and epigastrium, and prescribed a beverage of cream of tartar and cold water. In the afternoon I found that the fever was still high, that he had vomited repeatedly, was much distressed with nausea, and had been gently purged. The sinapisms were ordered to be repeated, the cream of tartar to be discontinued, and small quantities of iced water to be used to relieve thirst during the night; doses of 5 grs. quinine (in powder) were left, one to be taken in very little water at 4 o'clock the next morning, and repeated every two hours thereafter. I visited him at 8 A. M. and found that the fever had continued high during the night, and remitted only towards morning. He had taken 15 grs. quinine, and now had but little fever, although the nausea still persisted, and had caused him to reject the quinine twice, but which being repeated was finally retained. During this day the febrile exacerbation was much less intense, and he was kept on the use of iced water with a little lime water added to it. On the following morning, the nausea still being troublesome, and, apprehending that the quinine in solution or in powder would be rejected, I gave it to him in pills, 4 grains every two hours until he had taken 16 grains. These were retained, the nausea gradually subsided with the fever, and in the afternoon he was convalescent. He suffered a little from debility, but without further treatment, he was out in a few days. In this case head-ache and, gastric irritation instead of being increased, *subsided* under the use of quinine.

We are frequently called to cases in which we cannot ascertain the periods of exacerbation and of remission because of the ignorance of the patient or of his attendants, or because those periods are not very strongly defined. In such cases we may safely presume that the remission, if there be any, will occur in the morning, as this is most usually the case in these affections. And, under this presumption, I always prescribe about 20 grs. of quinine to be given in 5 gr. doses at intervals of two hours, commencing at the dawn of the next day, without regard to any incidental circumstances. This last injunction is added, because without it the attendant may upon some trivial change assume the responsibility of omitting the remedy at the only time when it might be given with decided advantage. I have known several cases to terminate fatally by such omission to carry out the prescription; the excuse being that the patient had too much fever, or head-ache, or nausea, &c. We not unfrequently see cases so late that the life of the patient depends entirely on our ability to prevent another paroxysm. No circumstance then must be allowed to interfere with the use of the only certain preventive with which we are acquainted. If it cannot be given in one form it must be given in another; if the stomach rejects it, throw it upon the rectum. At all hazards, give it. If by this course you happen to

give the quinine before the remissions have been fully established, it will not increase the fever, but on the contrary lessen its intensity, and consequently hasten the establishment of the remission. We frequently induce a very decided remission in cases in which it has previously been very slight, by the administration of quinine a short time after the fever has reached its acme of intensity, as may be seen by reference to the cases just related.

Having thus far restricted my remarks to the use of quinine in fevers uncomplicated with true phlegmasia or inflammation, it is proper that I say a few words in relation to cases we occasionally encounter, in which genuine phlagmasiæ are complicated with remittent fever or the paroxysmal peculiarity. I allude now specially to the form of Pneumonia and Pleuro-pneumonia which has prevailed more extensively in Georgia and South Carolina, (and perhaps in other southern states) during the last year or two than formerly, and which has been attended with an extraordinary degree of mortality. From what I have seen of such cases, and learnt from my professional brethren here, and elsewhere, I am satisfied that whilst the most striking element of the disease is an inflammation of the pulmonary organs, this is complicated with remittent fever. Indeed they present regular diurnal or tertian exacerbations and remissions of such decided character as to mislead the friends of the patient, and even his physician, into a degree of security which has often proved fatal. Seized with a violent attack of pneumonia, the patient finds himself at once quite ill, but is soon relieved from anxiety by an apparent amelioration of his condition. This continues until the next day, or perhaps the third, when another exacerbation supervenes and rapidly aggravates the condition of the lungs; but the intensity of the symptoms again abates, and the patient is flattered with the hope of approaching convalescence, until a repetition of the paroxysmal affection places his life in imminent peril, if not beyond the reach of remedial means—and all this notwithstanding a vigorous antiphlogistic course of treatment. This disease has been particularly fatal on our plantations, where the daily or tertian amendments of the patient have induced the owners or overseers not to call in medical aid as early as they would have otherwise done.

In all the cases of pneumonia, complicated as above stated, that have come under my observation, I have not hesitated to combine the use of quinine with that of the lancet, antimonials and opiates, and have uniformly had every reason to be entirely satisfied with the result. They do not require, nor can they bear, the same amount of depletion usually regarded as necessary in common pneumonia and pleurisy, and they very rarely yield to antiphlogistics alone. In furnishing my own testimony to the efficacy of the suggested combination, I might add that of other practitioners of distinction, who, entertaining the same views with myself, have met with similar success. It is scarcely necessary to add that the quinine should be given during the periods of remission, and as liberally as though there were no organ in a state of inflammation.

I have now freely and without reserve, given my views in relation to the use of quinine in our remittent fevers—and in lauding, as I have done, its efficacy, I cannot but apprehend that the charge of ultraism will be preferred against me by those who are still unacquainted with its properties. Be this as it may, I fear nothing from the test of time and experience, and will be amply compensated for the temporary odium, if this article will induce any who may have been backward in the use of quinine to give it a fair trial under the circumstances here recommended. It should be borne in mind, however, that we occasionally meet, even in this latitude, cases of typhoid fever, or of enteritic fever, in which quinine possesses no peculiar efficacy. But these fevers do not present the paroxysmal type, and can therefore be easily distinguished from those in which it is useful.—*Southern Med. and Sur. Jour.*

Excision of the Inferior Maxillary bone for Osteo-Sarcoma. By WILLIAM H. DEADERICK, M. D., of Athens, Tennessee.—The operation of which I propose to give a very brief account, was performed nearly thirty-seven years ago, and at that period, so far as I am informed, was unknown in surgery. Since that time it has been repeatedly executed, and the claim of having originated it has been set up by a foreign surgeon. By comparison of dates it will be seen that my operation preceded that of M. Dupuytren by two years.

On the 6th of February, 1810, Jesse Lay, a lad of about fourteen years of age, was brought to me on account of an excrescence which gradually arose from his gums, and which, in consequence of long neglect, completely enveloped the lower maxillary bone of the left side. It filled the inside of his mouth to such an extent as greatly to interfere with respiration and deglutition. Externally, the tumor exhibited the appearance of a wen of considerable size, and as it was daily augmenting it was evident that nothing short of its entire removal, with the portion of the bone it occupied, could save the life of the patient. Accordingly an incision was commenced just below the left ear, and continued along the course of the bone to the centre of the chin; a second one was made at right angles to the first. The integuments were then dissected from the tumor, and the bone sawed off at the angle of the jaw, and half an inch from the centre of the chin nearest the angle divided. The integuments were united in the usual manner, and the boy had a speedy and perfect recovery. The youth at the time of the operation, although fourteen years of age, was not larger than boys usually are at ten or eleven; but immediately afterwards commenced growing, and attained the ordinary stature of manhood. A well trained whisker hides, in a great measure, the scar left by the incision, and at a short distance the effects of the operation would not be observed.

Athens, Nov. 1st, 1846.

NOTE. Dupuytren is the generally accredited author of the operation above described. This distinguished surgeon removed a portion

of the lower jaw for a cancerous affection of the gums in 1812. The operation of Dr. Deaderick, it will be seen, was performed two years prior to that time. Dupuytren's case was reported to the Faculty of Medicine at Paris, by Lisfranc, in 1813. The report of Lisfranc is republished in the *Dictionnaire des Sciences Medicales*, vol. xxix. p. 430. Dr. Deaderick did not give to the public any account of his operation before 1823, when he described it in the American Medical Recorder.

Dr. Mott, in a letter to Mr. Liston, has preferred a claim to priority in this operation. He says, "I claim for myself and my country *originality* in the operation of exsection of the lower jaw at the temporo-maxillary articulation, and in different proportions for osteo-sarcoma. I avow and declare solemnly that before my first exsection of the lower jaw for osteo-sarcoma, I never saw, read or heard of anything of the kind ever having been done in any country." He adds, "We repeat and aver, that the exsection of the lower jaw of even a fourth part, much less a half or two-thirds of it, for any form of sarcoma involving the whole texture of the bone, has never in our opinion been performed by any surgeon, past or present, until by myself at the time above stated."

The operation of Dupuytren is admitted not to have been for osteo-sarcoma, but for a cancerous sore situated over the angle of the jaw, Ribes, in the *Dict. des Sci. Med.*, referring to this operation, has the following words: "These facts lead to the hope that fungus, or osteo-sarcoma of the lower jaw, a disease so formidable that it has in many cases been vainly attacked with the iron and fire, will henceforward, since the operation of M. Dupuytren be removed by amputation of a portion more or less considerable of the lower jaw without the danger of any accident, and, if the disease be local, with the certainty of success."

Many years before these predictions were uttered in Paris, the operation had been successfully performed by a young surgeon in the backwoods of Tennessee.

In a lecture delivered by Dr. Houston, of Dublin, in 1844, and published the same year in the London Lancet, the honor of having originated this operation is claimed for Mr. Cusack, who has performed it twelve times. The lecturer says, "The grand exploit of amputating the lower jaw, even from its articulations, the boldness of which has been only equalled by its success, has now become a standard operation in surgery. Persons afflicted with the distressing and loathsome disease for which this operation is undertaken, were formerly allowed to die, without any idea being entertained of the possibility of saving them; but now that a great mind, relying on a sound knowledge of the capabilities of the human frame, has set the example of extirpating the diseased mass *in toto*, many surgeons have fearlessly followed in the path thus laid open for them, and have derived honor from the success which crowned the enterprise. The success of this operation, both as regards immunity from danger, rapidity of convalescence, and the useful quality of masticatory apparatus which follows, is almost incredible."

Upon this passage Dr. Townsend, in his edition of Velpeau's Surgery, comments thus: "To whomsoever, therefore, the honour of this great triumph belongs, *mutatis mutandis*, the eulogium ought to apply equally well in Dr. Houston's conceptions, who, doubtless, would not desire to diminish one iota of it, because a name of different orthography from that of the justly respected Mr. Cusack, should happen to be found by a species of anaplastic substitution, to dovetail more completely than his with the historic facts in the case. We say cheerfully with all our heart *palmarum qui meruit ferat!*"

Dr. Deaderick's is the name which seems "to dovetail" most "completely with the historic facts," and to him, therefore, must the palm be awarded. True, he operated but once, and his operation was not made known to the world for many years afterwards; but it was undertaken for what appears to have been osteo-sarcoma; it involved the excision of nearly one-half of the lower jaw bone, and was crowned with perfect success. Dr. Deaderick did not call the disease *osteo-sarcoma*, but, in his account of his operation published in the Medical Recorder, described it as "a cartilaginous tumor." In the brief notice of it given above he applied no name to the affection, and the title prefixed to his communication is ours. Every medical reader knows how vague is the term "*osteo-sarcoma*," and what a diversity of morbid growths are called by that name. From the description of the tumor in Dr. Deaderick's case we have no doubt it would be styled osteo-sarcomatous.

It appears, then, that Dr. Deaderick preceded Dupuytren in the operation of excising the lower jaw bone two years, and that he anticipated Dr. Mott by eleven years, although he neglected to publish an account of the operation until after Dr. M. had communicated the results of his to the world; consequently Dr. M. was unapprised of what had been done by his countryman. He may still claim "for his country," if he cannot for himself, "originality in the operation," for Cusack's operations were performed two or three years subsequently to Dr. Mott's first. The operation has been performed by Dr. M. seventeen times. In a note appended to his letter to Mr. Liston, Dr. Deaderick's operation is referred to, and this brief, obscure notice is all the allusion to it that we have found in looking through the American edition of Velpeau's great work on surgery. We have deemed it but an act of justice to a modest and worthy member of the profession to give these dates in connexion with the history of his case.—*Western Journal of Med. and Surg.*

A large Tumour of the Mamma, spontaneously cured. Reported by A. B. GREENE, M. D., of Sumter County, Georgia.—The subject of these remarks is a negro woman belonging to Col. M. A short time after she arrived at the usual age for the full development of the sexual organs, the right mamma was observed to be largest, and increased in dimensions gradually and continuously, unattended with pain, except what was induced by its suspension, when not properly supported. About twelve months preceding her pregnancy, my at-

tention was first called to an examination of the enlarged mamma, and I found all the usual evidences of simple sarcoma present, with nothing of a malignant character. The skin and subjacent tissues seemed as healthful to the touch, and as free from uneasiness when handled, as any other mass of flesh about the body. Its size already amounted to great deformity, and was obvious to any passing observer.

I did not hesitate to advise her intelligent owner, that an immediate operation was the only certain cure; and pointed out the danger should amputation be deferred, that might result from pregnancy and accouchement, in the development of some malignant tendency, or at least, subjection to severe suffering.

The force of my suggestions was appreciated; but fearing the operation, and trusting to some happy, fortuitous change, it was neglected. She became pregnant, and at the full term of gestation in January last was delivered without any unusual difficulty, and soon felt pain in the enlarged breast for the first time. At the expiration of six weeks, it had grown so rapidly, that while sitting upright the enlarged mamma rested on her lap. It was enormously distended, and of such weight, that however well supported, her chest was strongly inclined forward. I could never detect any circumscribed induration, nor any other evidence of the formation of abscess. She was put upon strict antiphlogistic treatment, medicinal and dietetic. General and local depletives, emollient fomentations and poultices, to relieve turgidity; and the occasional application of the pump and cup to the nipple to encourage a discharge of the milk, should any possibly be secreted.

While these means were being applied, a spontaneous discharge occurred from about an inch above the nipple—in a continued stream it incessantly poured out, until one gallon and a half of a milk-like fluid had come away. Immediate and complete relief was expressed, and no appearance of debility, or other adverse symptom, as a consequence of the sudden and almost incredible quantity dislodged, was observed.

From that time, the breast gradually assumed the size it had attained before her accouchement. Her general health and strength improved daily, and in a short time she was out on the farm at labor, though of an easy kind, and self-imposed. While thus employed, she was exposed to a drenching rain, and in a few days I was summoned to visit her again.

I found her now labouring under an acute rheumatism, in its most dangerous and painful form. The pleura was implicated, and a manifest tendency to low typhus rendered her condition very critical.

When we found that she might possibly survive the attack, my attention was again directed to the condition of the diseased breast. Upon examination an ulcer was found, situated near the nipple—the breast again greatly increased in size. The ulcer discharged matter of a thin purulent character, and most noisome odour. Other ulcers were formed, and they soon numbered seven, emitting vast quantities

of insupportable horrid pus-like fluid. By patient perseverance in the use of the chloride of lime, charcoal poultices, and a variety of astringent injections, the discharges assumed a more healthy appearance and less offensive odour.

The patient was now a miserable looking object of emaciation; and to every one her death seemed inevitable. It would be needless to dwell upon the treatment here, constitutional and local.

The largest and most pendent ulcer presented a prominence in the centre, which I found to be insensible. Upon raising it with the forceps, I was surprised to find that by gentle traction a considerable mass was drawn out without any pain. I applied the scissors and separated that portion. There was not a drop of blood discharged. I then seized the remaining mass with a small tenaculum, and readily drew out in a string-like form, a connected volume of flesh coloured substance, until by the pain produced, I supposed the mass to be too large to pass through the ulcer, and again detached it with the scissors.

I now found a large hollow chasm left under the ulcer, and changed the position of the breast to examine the other ulcerated openings, but as they were situated above, nothing unusual presented at them; but when restored to the same situation again, I found the chasm occupied, and another mass presenting. By pulling gently with the forceps at different points, I succeeded in starting another slough through the ulcer, which passed easily, until one entire mass of large size fell from it without the use of any excising instrument. No blood followed, except what escaped from the edges of the dilated ulcer.

There was now an extensive, unoccupied space, left by the large volume of detached slough; and by the aid of the light admitted through the several ulcerated openings, (I fancied,) was seen the original "bona fide" gland, bounding the upper part of this space.

As nature had performed the amputation so well, thus far, I determined not to interfere, in the already exhausted condition of the patient, by removing the now redundant skin.

Our next care was to ascertain the amount of the self-amputated mass, which was attached to the hook of a pair of "draw-spring steel-yards, (the only means of weighing at hand) and weighed one and a half pounds. The small fragments, not weighed, Col. M. supposed would amount to at least a half pound more.

We were surprised, at first, that so much bulk and volume should weigh no more; but upon careful examination, I found it composed of a cellular structure, comparable to honey-comb, interspersed with fine particles of a "coffee-ground" colour. The breast was bathed with infusion of red-oak bark, and covered with a large poultice of the same excellent astringent, moistened with solution chloride of lime, and secured by means of bandages, bound on so as to exert due compression on the now flaccid parts. General directions were given for future management, and I did not see the patient from that time, which

was 24th of April last, until a few days since, when I visited her, in order to report upon her present condition.

She has been doing the labour of a hand upon the farm, for some two months now. The remains of the mamma are about half the size of the well-formed natural one—the skin is regularly contracted and adherent—the ulcers are all healed soundly, except a very small perforation, still giving exit to some discharge, of inoffensive appearance. Her general health is perfect in all respects, having recovered her former flesh, activity and cheerfulness of mind.—*Southern Med. and Surg. Journal.*

Remarks on the use of the terms "Congestion" and "Congestive."
By THOMAS C. BROWN, M. D., of Woodville, Miss.—The term *Congestive Fever* has become so familiar in the South and West, that the young and inexperienced are led to look for a distinctly marked and peculiar fever, and are sadly perplexed in their investigations. At one time it is described as "algid intermittent fever," at another as "malignant or pernicious remittent fever." They find it supervening on intermittent, remittent and continued fevers—on gastritis and gastro-enteritis—on typhoid pneumonia—on epidemic dysentery; and in fact, as complicated with most of our summer and autumnal diseases, and sometimes with our winter epidemics.

Would it not be well for the medical profession, if our journalists and teachers of medicine, would teach that "there is not, properly, any fever characterized by *congestion* in contrast with another which is without it;" but that *congestion* is common to nearly all fevers, whether intermittent, remittent, or continued; and that it is not limited to any one organ; the brain, the spinal column, the lungs, the liver, the spleen, the kidneys and abdominal viscera, may be the seats of congestion, either separately or several of them at the same time; and these as they are separately or severally affected, will characterize the symptoms according to the organ or the several organs which may be involved?

The term *Congestion* comes from the Latin *congero*, to amass, and in a medical sense conveys the idea of turgescence or fulness of blood in a particular part, and implies simple engorgement and over distension of the blood vessels. It seems rather the result of irritation than of inflammatory action, and when it passes into inflammation, it would seem to be more dependent on the general state of the constitution, on the plethoric condition of the blood vessels, with the tendency towards inflammatory excitement, than on the primary irritation which occasioned it. The presence of internal congestions must, of course, be inferred from the symptoms which indicate them, and to connect the symptoms with the structural lesions which they denote, so as to furnish practical guidance in the treatment of them, is the proper object of medical research.

Their treatment must depend in part on the nature of their site, but principally on the cause which occasions them. If congestion

result from a plethoric habit, mere local treatment can never give permanent and effectual relief; on the contrary, if the congestion be owing to irritation only, then general depletion is unnecessary, and would be injurious.

Congestion generally precedes, but may follow inflammation, by the irritation set up in neighbouring organs, or be transmitted to those more distant by nervous influence. Strong innervation, in which the nervous centres are much excited as under various emotions, will produce congestion, at one time of the brain, at another of the lungs or liver. Certain nervous affections, as hysteria, will give rise to irregular determination and retardation of blood, producing congestion in some organ, which often entirely disappears after the removal of the nervous disturbance. Congestion dependent on mere nervous disorder, will generally be irregular or periodical in its appearance, but if there be a permanent disorder of function in an organ, the congestion depends upon permanent irritation. In the commencement of fevers the congestion is of an irregular or periodic character, but when occurring in the latter stages of fever, it is usually of a more permanent and dangerous character.

Congestion, coming on in the progress of intermittent and remittent fevers, is of frequent occurrence, and when supervening in the latter stages of these diseases, it generally indicates great danger; but when it ushers in an intermittent fever, and the paroxysm is followed by very feeble reaction, it indicates great danger.

The symptoms of the congestion of a particular organ, or of several organs, combining with the sympathy of a particular fever or disease, upon which it supervenes, must, necessarily, give rise to great diversity of symptoms, and will require a variety of treatment as dictated by the existing circumstances. Hence the confusion which results in an attempt to study the symptoms of congestive fever, as a distinct species of fever. If we are to have a *nosology*, in which *congestion* is to be the cognomen of a particular species, we must adopt a division of fever somewhat like the arrangement of Armstrong, and dispense with the distinctions of fever into intermittent, remittent and continued.—*New Orleans Med. and Surg. Journ.*

Blindness Caused by the Use of Sulph. Quinine. By JOHN McLEAN, M. D., Prof. of Materia Medica in the Rush Medical College. —Quinine when freely administered produces a species of intoxication, tinnitus aurium, a sense of fulness in the head, cephalagia, and other affections; and sometimes, although not so frequently, blindness, more or less lasting.

M. Trousseau, relates the case of a tailor, who, for the relief of a periodical asthma, took 48 grs. of the sulph. quinine, at one dose. In four hours he experienced ringing in the ears, dulness of the senses and vertigo; and in seven hours, he was blind and deaf, his mind wandered and he was unable to walk. These effects, for which no active medicine was administered, gave way spontaneously, during the night. A young girl at the "Hospital Cochin," in consequence

of having taken freely of the sulph. quinine, became affected with amaurosis, which continued at the end of three weeks, notwithstanding appropriate and energetic means were employed for the restoration of her sight.

Dr. Rognetta, who claims for Rasori, priority in the use of quinine in acute rheumatism, "thinks, with the Italian physicians 'that the limits of tolerance should not be exceeded, and that beyond this, a species of poisoning may be induced, known by deafness, blindness, hallucinations, hæmaturia, &.'"

Blindness, although not so common as the other effects, is not unfrequently produced, and may be prolonged for months or even years. It is not, however, generally known, that such may be the result of this medicine, when given in large quantities. The following are some cases occurring in this place and immediate vicinity, which show that when thus administered it may produce blindness more or less permanent.

Case 1st. Mr. P. of the town of Barry, Jackson co., was in the year 1840 attacked with a low grade of remittent fever, the nature of which was such, as to cause the attending physician to administer the sulph. quinine in large and frequent doses. Sixteen grains, (as judged by sight,) were ordered every hour, and continued until nearly one ounce was taken. Before the quinine was discontinued, he became perfectly blind, which, with a slow and gradual amendment, continued during the first year. Later than this, I have not been positively informed in regard to the case, but should judge, from what indirect information I have received, that his sight is not yet perfectly restored.

Case 2d. Mrs. B., of the town of Concord in this county, was, a few years since, reduced so low by the endemic fever of the country, that her life was despaired of. As a last resort large quantities of quinine were given, and while taking it she became blind, which continued for several weeks. As she recovered her health the blindness gave way, and her sight was finally restored. Not being acquainted with the particulars of this case, I can give but these few general outlines.

Case 3d. P. M. Everett, of this place, was, in the autumn of 1843, attacked with remittent fever, and in a few days became so greatly reduced, as to leave but slight hopes of his recovery. Sulph. Quinine was therefore prescribed, in doses averaging three grains, every hour, and was continued for three days. In a short time, he became deaf, and soon after so blind that he could not see a burning candle, when placed immediately before his eyes. The blindness took place on the third day, after the commencement of the free administration of the sulph. quinine. Previous to this, and at this time, his mind, (with the exception of occasional slight wanderings) appeared to be perfectly clear. After some weeks, his sight became partially restored, but continues more or less imperfect, even at the present time.

During the greater part of the first year, he could look steadily at

the sun, without seeing it, or even any painful sensation being produced. When he first began to see sufficiently to read, which was in the course of the first year, he could perceive but a small luminous spot upon the paper, about one inch in diameter, within which he could distinguish letters, but all without this was cloudless and confusion. During this time, the pupils were very much dilated, and he could see objects at a distance much better than those near by. His sight has continued to improve, ever since; and at the present time, although quite imperfect, is sufficiently good to enable him to read and write, although with some difficulty. The pupils are still considerably dilated, and it is with great difficulty, that he can discern objects by twilight. The direct rays of the sun upon the head, produce pain there, accompanied with a painful sensation deep in the orbit of the eye, and a disordered vision. At the present time, exercise easily produces fatigue, by which his sight is much impaired.

Case 4th. In the month of April, 1846, Dr. R. of this place took in doses of six grs. each, three drachms of quinine in 36 hours; at the expiration of which time, he became perfectly blind. His hearing was somewhat blunted, although it did not, in degree, equal the blindness. On the two succeeding days, his sight, although very imperfect was considerably restored. Had he lived, the probability is, that this imperfect sight would, as in the former cases, have continued a considerable length of time.

Remarks.—We think it clear that the blindness in the foregoing cases was the effect of the quinine; for we see it in each, coming on suddenly during its administration in large quantities, and at a time, when no other medicine was given that would be likely to produce such results. Here, cause and effect appear to be closely connected, and are so plain, as scarcely to admit of the possibility of a doubt. From the symptoms accompanying the foregoing cases, we judge that the proximate cause of the blindness, was mainly an affection of the retina or optic nerve, producing amaurosis.

I have recorded the foregoing facts, with the hope that they might be the means of causing some useful suggestions, in relation to the physiological effect and administration of this medicine.

In connection with the foregoing, we might mention the case of Mr. B. Porter, of this town, who has had for sixteen years and upwards, amaurosis of the left eye, which he supposed to have been produced by the application of a strong subacetate of copper ointment to that side of the face, for the purpose of curing *Herpes circinatus*. As the ringworm gave way, the blindness came on.

About one year since, he suffered with a periodical neuralgia, for which I ordered 32 grs. of quinine to be taken in divided doses of 4 grs. each, every two hours. Under its influence, the neuralgia disappeared; and on the following day, he could see objects quite distinctly with the amaurotic eye, much better than ever before, since it first became diseased, and he was much elated with the thought

of soon regaining its sight. He, however, took no more quinine, and in a few days, the benefit produced to that eye was entirely lost.

Jackson, (Mich.) Sept. 22, 1846.

Illinois and Indiana Med. and Surg. Jour.

On the Effects of Mercury on the Young Subject.—By JOHN B. BECK, M. D., Professor of Materia Medica and Medical Jurisprudence, in the College of Physicians and Surgeons, of New York. In some previous papers, I endeavoured to point out the peculiarities attending the operation of Opium and Emetics, on the infant subject, as distinguished from the effects of these agents on the adult. I now propose to make some remarks on another article of even still greater importance, and that is *Mercury*. That Mercury is an agent of immense power, either for good or evil, upon the human constitution, cannot be questioned. While in many cases it is the means of saving life, in not a few it unquestionably destroys it. If this be so, it becomes a question of the deepest practical interest, to determine whether its action is modified in any way by the age of the patient, and particularly so, when it is recollected that it is given by too many physicians, even more freely, and may I not add indiscriminately, to the young subject than to the adult.

The first and most striking peculiarity attending the action of mercury is, that in young subjects, it does not produce salivation so readily as it does in adults. Indeed, under a certain age, it appears to be exceedingly difficult to excite salivation at all in them. On this point, besides our own experience, we have abundance of testimony. Dr. Clarke says, "under various circumstances he has prescribed mercury, in very large quantities, and in a great number of cases; and he never produced salivation, except in three instances, in any child under three years of age." Dr. Warren, of Boston, observes, "that he has never known an infant to be salivated, notwithstanding he has given in some cases, large quantities with this view." Mr. Colles, of Dublin, says, "no man in the present day requires to be told that mercury never does produce ptyalism, or swelling and ulceration of the gums in infants." Drs. Evanson and Maunsell speak still more strongly. They say, "mercury does not seem capable of salivating an infant. We have never seen it do so, nor are we aware of any such case being on record." "We have never succeeded in salivating a child under three years of age."

The same general fact seems to be applicable to the external use of mercury. Dr. Percival, of Manchester, remarks, that he "repeatedly observed that very large quantities of the Unguentum Cæruleum may be used in infancy and childhood, without affecting the gums, notwithstanding the predisposition to a flux of saliva, at a period of life incident to dentition."

That salivation does not take place so readily in the infant as in the adult, would seem then to be well established. That it never can or does take place, as might be inferred from some of the preceding quotations, is by no means, however, true; and the statement,

if implicitly relied on, is calculated to be the cause of much mischief. That very young subjects do sometimes become salivated, is unquestionable. One case, and only one, however, has occurred in my experience, in which a child of two years of age was salivated, and that by a very moderate quantity of calomel, viz., five grains, given in three portions, at intervals, within the space of about twelve hours. In about two days after, the gums became inflamed, the tongue swelled, several ulcers appeared in the mouth, and the flow of saliva was free; after continuing about three days in the same state, it gradually yielded, and disappeared without any further inconvenience. In this case every thing seemed favourable to the developement of mercurial action. The child had been labouring under whooping cough for several weeks, and was a good deal reduced. It vomited freely with every paroxysm of coughing, and this no doubt aided in bringing on salivation, in a constitution peculiarly sensitive and evidently scrofulous. Nor is this a solitary case. Dr. Clarke, already quoted, admits that in three cases salivation was produced in children under three years of age. And similar cases have been observed by others. Dr. Blackall relates the case of a child, two years of age, who was salivated in consequence of taking two grains of calomel for several successive nights. The child was a poor scrofulous subject, and it sunk under the effects of the mercury.

This, then, is a remarkable peculiarity in the action of this agent upon the infant subject, and the observation of it has doubtless led to the belief, too prevalent among some physicians, that it may be given to them to almost any extent with perfect impunity; an error, which, if not in its immediate, yet certainly in its remote effects, has been the prolific source of more mischief, probably, than any of us are aware of.

Although mercury so seldom salivates infants, yet, notwithstanding this, it cannot be doubted that it affects the system profoundly, and even more so proportionally than it does the adult. That it should do so appears perfectly natural, when we reflect upon the mode of its operation on the human system. On this subject, I am aware that a great difference of opinion exists. By some, mercury is looked upon as a stimulant; while others view it as a sedative. A familiar acquaintance with its effects, however, will show, I think, that it may be the one or the other, according to circumstances—according to the dose in which it is given—the length of time it is continued, and more especially, the condition of the system at the time of using it. A single large dose of calomel will cause nausea and relaxation, and sometimes unpleasant prostration, while if it be given in smaller doses and repeated frequently, it will occasion irritation of the intestines, and general disturbance of the vascular and nervous systems. In the former case acting as a profound sedative, and in the latter as a stimulant, or rather irritant. That calomel given in large doses operates as a sedative, seems to be proved, not merely by the nausea and prostration which it frequently produces, but by other considerations. In dysentery, for example, in the adult, a dose of twenty grains of calomel will sometimes allay pain and irritation,

with as much certainty as a dose of opium. For the purpose of testing the effects of calomel, some interesting experiments were made by Mr. Annesley, which would seem still further to show, that in large doses the action of this agent upon the mucous membrane of the stomach and intestines, is that of a sedative. He took three healthy dogs, and gave to one, $\mathfrak{z}\text{j}$. of calomel, to a second, $\mathfrak{z}\text{ij}$., to a third, $\mathfrak{z}\text{iiij}$. After this they were tied up in a room.

"The dog which took $\mathfrak{z}\text{j}$. did not appear to feel any kind of sickness, till six or seven hours afterwards, when he vomited a little. He was lively the whole time, and ate his food well; had been purged two or three times; dejections of a black grey colour.

The dog which took $\mathfrak{z}\text{ij}$. was likewise lively, and ate his food well, vomiting two or three times, and was purged more than the other; he passed tape worms and the dejections were black.

The dog which took $\mathfrak{z}\text{iiij}$. was heavy, and apparently uncomfortable the whole day, and did not vomit at all; he was purged, and passed a very long tape worm; dejections also black."

Twenty-four hours after they had taken the calomel, the dogs were all hung, and five minutes after they were dead, they were examined, and the vascularity of the stomach was found to be in the inverse ratio of the calomel they had taken; i. e. in the dog which had taken $\mathfrak{z}\text{iiij}$., the vascularity was the least, and so on. For the purpose of comparing this with the condition of the stomach of a dog which had taken no calomel at all, an examination of another dog was made; and here the stomach was found to be *more vascular* than in any of the others. From these experiments, Mr. Annesley drew the conclusion, that "the natural and healthy state of the stomach and intestinal canal is that of high vascularity, and that the operation of calomel in large doses, is directly the reverse of inflammation."

The foregoing considerations would seem to show that calomel in full doses is a local sedative, and in its general effects, is debilitating to the system at large. Hence its great utility and value as a remedy in many inflammatory diseases.

When, on the other hand, it is given in small and repeated doses, it acts not unfrequently as a local, as well as a general irritant, producing immoderate action of the bowels, and general irritation of the nervous and vascular systems. Now these, we know, are the effects observed continually in the adult, and it is but reasonable to suppose that all of them must, as a matter of course, be aggravated in the more delicate and sensitive system of the infant.

What shows incontestibly that the action of mercury is actually more energetic on the infant than the adult, is the fact, that when salivation does take place in the former, as it sometimes does, its effects are most disastrous. Sloughing of the gums and cheeks, general prostration, and death are by no means uncommon occurrences. On this subject, Dr. Blackall justly remarks, "a general opinion prevails, that the constitutions of young subjects resist mercury. Its entrance into the system they certainly do resist, more than we could expect; but they are greatly overcome by salivations, and the possi-

ble occurrence of such accidents may well set us constantly on our guard." Dr. Ryan, too, says, "Ptyalism of infants is often followed by sloughing of the gums and cheeks; and this I have known to occur after the use of it in Hydrocephalus."

Besides being more energetic in its action on the infant, mercury is also more uncertain. This must necessarily be the case, and for the same reasons that every other active agent is so. In the adult we know that mercury varies in its effects, according to the condition of the system, and the peculiarities of the patient's constitution. Thus some persons are salivated by the smallest quantity of this metal, while others resist the influence even of the largest quantities. In some, febrile action; in others, diarrhœa and exhaustion take place even from moderate doses. Hence it is, that every prudent physician, if unacquainted with the previous history of his patient, makes it a special subject of inquiry to ascertain whether he has ever taken mercury previously, and how it affects him. Now, in the young infant, of course, as we cannot so well have the benefit of this information, more uncertainty must necessarily attend its operation.

These, then, are the peculiarities attending the operation of mercury on young subjects, viz: that they are salivated with great difficulty, and that notwithstanding this, the effects of it are frequently more energetic and uncertain, than they are in the adult. And it is upon these as the basis, that I propose to make a few remarks bearing upon the practical application of it in young subjects.

1. If salivation occurs so rarely in children under a certain age, then it is evident that it can never be made a criterion by which to judge of its influence on their systems. To attempt, therefore, to produce this effect, as we do in adults, is manifestly improper. In cases where it is desirable to get the system under the full influence of the remedy, other modes must be resorted to for the purpose of judging to what extent the use of the article should be carried. Now this is by no means easy. Even in adults, where we have the benefit of salivation as a test, all practical physicians are aware how difficult it is frequently, to decide when it is proper to stop the use of the remedy. How much more so must this difficulty be increased in the young infant, where we are left without this guide. The only modes of judging, of course, are the character of the evacuations from the bowels, and the general impression made upon the disease for which it is administered. Both these are evidently, however, uncertain. It is to be feared, therefore, that for the want of a more certain guide than we at present possess, the use of this remedy is, in many cases, unnecessarily protracted to the great detriment of the little patient. From all this the conclusion is obvious, that in the use of this article in the young subject much greater caution is necessary than in the adult.

2. The fact that mercury may prostrate and destroy a young child, even though it does not cause salivation, it is to be feared is not sufficiently appreciated, at least by some. We have known calomel given without weight or measure, to a young child, and the reason

assigned to justify it was, that it could do no harm, because it would not salivate. Now it appears to me that no opinion can be more unfounded, and no practice more mischievous. Although a single dose of calomel, even though large, may be well borne by children of ordinary strength of constitution, yet even this is not entirely safe in all cases. And when these doses are frequently repeated, particularly in delicate habits, the most serious consequences may result.

3. The use of mercury in young subjects as an alterative, should in all cases be conducted with great caution. There is no practice more common than that of continuing the use of this agent in small doses, for a considerable time, and certainly none which is more liable to abuse. Under the idea that the dose is so small and from no salivation appearing, we are apt to infer that even if the medicine is not doing any good, it is certainly not doing any harm. Any improvement, too, which occurs during the use of the article, is sure to be attributed to the silent operation of it on the system. Now although this is not unfrequently the case, yet it is not invariably so; and every observing physician must have been aware of cases, in which, in this way, the article has been unnecessarily and injuriously continued. In bowel complaints, under the idea of altering the secretions, it has frequently, no doubt, helped to keep up the very intestinal irritation which it was given to correct. In other cases it has developed the latent tendency to other diseases, such as Scrofula, Phthisis Pulmonalis, etc. In adults we know this to be very often the case. How much more likely is all this to happen in the young infant.

4. In the use of mercury in young children, great care should be exercised in ascertaining, as far as possible, their constitutional peculiarities. This, of course, is not in all cases easily to be done. A good deal, however, may be learned from an acquaintance with the tendencies of the parents. Whenever the parents show indications of scrofula, or where there is an hereditary predisposition to consumption, great caution ought to be exercised in the use of mercury in their offspring.

5. Mercury should be administered with great caution, in cases where a child has been sick for a considerable length of time, and when the strength of the child has been very much reduced. In this state of constitutional depression, a single cathartic dose of calomel sometimes proves fatal. We think we have seen more than one case, in which a child has been irretrievably prostrated under these circumstances, under the false impression that calomel is an innocent purgative to a child.

6. The too common practice of giving calomel as an ordinary purge, on all occasions, is certainly unjustifiable. From the facility with which it may be given, it is unquestionably resorted to in a great number of cases, where it is certainly unnecessary, and in a great number where it positively does harm. The misfortune is, that its use is not limited to an occasional dose, but it is too often given in every slight indisposition of the child. Now, in this way, there

can be no question that the use of it has laid the foundation for the ruin of the constitutions of thousands. It ought to be a rule laid down and rigidly followed, that in very young children, mercury ought never to be used as a cathartic, unless there is a special reason for resorting to it. In a great majority of cases, milder cathartics are decidedly to be preferred.

In concluding these observations, I trust it may not be supposed, that my intention has been to undervalue the importance of mercury as a remedy in the diseases of children. On the contrary, no one appreciates it more highly than myself. In many cases, nothing can supply its place, and its judicious use has been, and is, the instrument of saving multitudes of lives. Notwithstanding, however, the many cautions to the contrary, it is to be feared that the use of it is still too general and indiscriminate. Indeed, the amount of it which is taken by the human race in one way or other, is incalculable. What is given by regular physicians, is perhaps the smallest quantity. If the public really knew how much of this article is swallowed unknown to themselves, in the shape of bilious pills, worm lozenges, and the white powders of the Homœopaths, they would be amazed at their credulity in deserting their old medical advisers, because they have the boldness to give them an occasional dose, and the honesty to tell them so.—*New York Annalist*.

Remarks on Strangulated Umbilical Hernia, with a Case, by A. J. WEDDERBURN, M. D., Professor of Anatomy in the Medical College of Louisiana.—Strangulated umbilical hernia, being an affection of very rare occurrence in the adult male, it is deemed proper to report the following case for which a successful operation has been performed.

A negro man of 300 pounds weight, aged 31 years, the property of Dr. Slone, residing about two miles below New Orleans, states that he has always had an umbilical hernia, easily reducible, and about the size of a hen's egg—was attacked with a pain in the abdomen—found the tumour enlarged—attempted its reduction, but failed. I saw this case about 12 hours after the strangulation occurred, and found the tumour about the size of a child's head, very tense and elastic. About three hours before I saw the case Dr. Slone had used the various means recommended for reduction in such cases, and on my visiting the case with him, we further endeavoured for the space of an hour, to effect it by taxis, and to aid in the attempt we administered tartar emetic, tobacco injections, large injections of cold water with a hydrostatic injecting tube, and applied ice and sulphuric ether to the tumour. These efforts having failed, we determined upon an operation as the only means calculated to afford the patient a chance for his life. A short time previous to the operation, whilst making forcible taxis, a considerable portion of gas contained in the incarcerated bowl escaped with a gurgling sound, and the tumour seemed to subside so rapidly under my hand, that I was induced to believe that I had succeeded in the reduction, but in a few minutes was con-

vinced that the intestine could not be returned, or even the escape of more of the gas effected. By this effort the tumour was reduced so much by the escape of air from the intestine, and the tension consequently so much relieved, as to lessen the danger of wounding the intestine in dividing the integuments. We informed the patient of the necessity of an immediate operation, to which he somewhat objected, and only consented after we had allowed him to make an effort at reduction himself, which he continued for something like ten minutes, using all the while considerable and well directed force, for he had returned the intestine so often himself when the hernia was smaller and reducible, that he had acquired quite a degree of dexterity in the matter.

Operation.—An incision was made upon the top of the tumour three or four inches in length through the skin in a vertical direction, so as to expose the superficial fascia, which was considerably condensed. A small opening was made through the fascia at the lower part of the wound, by light and careful touches with a scalpel, into which a director was introduced, and carried to the upper part of the first incision, and the fascia divided to correspond with the same, with a probe pointed bistoury. I then introduced my finger between the peritoneal sac and the superficial fascia, and carrying it in every direction, easily effected the separation of the two membranes. Finding there was not room to work in, the wound was enlarged by extending it about an inch above, and more than an inch below,—this enabled me to turn aside the integuments, and obtain a good view of the sac, which contained, floating in a nearly transparent fluid, a large portion of the omentum, and as well as I could determine, about fifteen inches of the small intestinal tube, which appeared to be perfectly black. A small opening was next made in the lower part of the sac, by seizing up a small portion of it with a pair of artery forceps, and carefully cutting in a horizontal direction. After the escape of about four or five ounces of fluid, a director was introduced, and the sac divided upon it about four inches. In a few moments the intestine, which presented the dark colour before mentioned, began to assume a red appearance, from the action of the air acting upon the blood contained in its vessels, through the coats of the intestine; from which circumstance, and also from a close examination, I was convinced that the gut retained its integrity, and at once endeavoured to return it by an attempt to kneed in a small portion at a time;—but in my efforts I not only failed, but additional portions of the omentum and intestine were forcing themselves from the abdomen. In order then, that the stricture could be arrived at without endangering the gut, it became necessary to extend the incisions, not only in the integuments nearly to the base of the tumour, but also in the peritoneal sac. The finger was then forced into the umbilical ring, and a probe pointed bistoury passed by its side, with which the structure was divided at the lower part about the fourth of an inch. An attempt was again made to return the bowel, but without success, when a division of the ring was made at the upper part, less

than a fourth of an inch in extent, by which means I was enabled to return the bowel by pushing in a small portion at a time. After returning the intestine, the omentum was returned, and spread out in front of the small intestines, as well as it could be done by the introduction of my finger into the cavity of the abdomen for this purpose.

After returning the contents of the sac, the blood was sponged from the wound, then leaving it open for the space of about 15 minutes, in order that the oozing from the divided vessels might be entirely arrested, the edges of the wound were brought in contact, and three interrupted sutures applied. The skin, which was very loose, was gathered up like a bag, with the sack contained within, but not included in the ligatures, and a graduated compress applied, with the hope that the mass would, if healed, be a barrier to a further protrusion, and effect a radical cure.

The operation was performed on the 23d of September last, and the patient recovered without a bad symptom. Small doses of calomel and opium were administered during four or five days. In ten days after the operation the patient walked across the room and is now entirely well, a radical cure having been effected.

As the operation just described differs somewhat from those recommended by different surgeons, I have thought proper to give below a few extracts, with some general remarks on the subject of exomphalos.

In Cooper's Surgical Dictionary we find the following: "In consequence of the great fatality of the usual operation for the exomphalos, I think the plan suggested and successfully practiced by Sir A. Cooper in two instances, should always be adopted when the tumour is large and free from gangrene; a plan that has also received the high sanction of that distinguished anatomist and surgeon, Professor Scarpa. (*Traité des Hernies*, p. 362.) Perhaps I might safely add, that when the parts admit of being reduced, without laying open the sack, this method should always be preferred. It consists in making an incision just sufficient to divide the stricture, without opening the sack at all, or, at all events, no more of it than is inevitable."

In umbilical hernia, of not a large size, Sir A. Cooper recommends the following plan of operating: "As the opening into the abdomen is placed towards the upper part of the tumour, I began the incision a little below it, that is, at the middle of the swelling and extended it to its lowest part. I then made a second incision at the upper part of the first, and at right angles with it, so that the double incision was in the form of the letter T, the top of which crossed the middle of the tumour. The integuments being thus divided, the angles of the incision were turned down, which exposed a considerable portion of the hernial sack. This being then carefully opened, the finger was passed below the intestines to the orifice of the sack at the umbilicus, and the probe-pointed bistoury being introduced upon it, I directed it into the opening at the navel, and divided the linea alba

downwards to the requisite degree, instead of upwards as in the former operation. When the omentum and intestine are returned, the portion of integument and sack which is left, falls over the opening at the umbilicus, covers it, and unites to its edge, and thus lessens the risk of peritoneal inflammation, by more readily closing the wound.

In Gibson, page 128, we find the following: "Strangulated umbilical hernia very frequently proves fatal, as much from disorder of the intestinal function as from the strangulation. When the usual remedies fail, an operation should be resorted to. This may be done in the following way. An incision, several inches long, is made very cautiously through the integuments and superficial fascia, when the sack, if not absorbed, as it often is, will appear. Into this a small opening should be made, from which fluid in considerable quantity generally issues. The opening may then be enlarged, and a finger carried upwards between the omentum and intestine as high as the umbilical ring. Upon the finger a bistoury is next to be carried through the linea alba, to the extent of an inch, which, in most cases will relieve the stricture sufficiently to enable the operator without much difficulty to restore the parts to their former situation.

"Dr. Physick has proposed, in strangulated umbilical hernia, to make a crucial incision through the integuments, as far as the neck of the sack, then open the sack at its upper part to an extent sufficient to enable the operator to examine its contents, and reduce them, if possible, without dilating the umbilical ring. Should the latter expedient, however, become necessary, the stricture must be divided on the outside of the sack. After the omentum and the intestine are restored to the abdomen, a ligature should be drawn around the neck of the sack, with a view of closing the cavity and obviating peritoneal inflammation. The late Dr. Wistar once performed the operation with success. In the case of a Mrs. N., a very respectable Jewish lady, I performed a similar operation about fifteen years ago. The tumour, however, was as large as a child's head, and had been strangulated several days before I saw the patient, and, on this account, the operation did not succeed. The patient, too, was advanced in years, extremely corpulent, and had long suffered from derangement of the functions of the stomach and intestines. Under these circumstances, no operation, probably, would have answered the purpose, even if performed in the very commencement of strangulation."

In Lawrence, on ruptures, we find the following:

"The greatest practical writers have strongly represented the frequent fatality of the operation for strangulated exomphalos; and the results of my own experience coincide entirely with their statements. I have, indeed, operated successfully on a large intestinal exomphalos, containing several convolutions of small intestine, of a bright red colour, without any omentum, in a fat woman advanced in years; but the majority of cases, in which I have either operated myself, or seen the operation done by others, have ended fatally.

"Perhaps this fatality may be in some degree explained by considering that the exomphalos is most frequently in fat gross subjects, unfavourable for operations; that general intestinal disorder either exists with rupture, or is speedily produced by it; and that irritation and inflammation are readily propagated to the stomach, which is close to the umbilicus."

In the same work we find the following: "The great fatality of the ordinary operation for exomphalos makes it advisable that we should employ every precaution calculated to diminish subsequent irritation and inflammation. Hence it would be proper to adopt, especially if the tumour exceed a moderate size, the mode of operating which is applicable to large inguinal herniæ; in which the tendon is divided without opening the sack; or the latter part is only cut sufficiently to allow the division of the structure. This will permit the return of the parts if they are not adherent; and if adhesion should have formed, the immediate cause of danger, the strangulation is removed. The approximation of the sides of the wound by sutures, or adhesive plaster, will prevent the occurrence of inflammation in the tumour. The practicability of this mode of operating in umbilical ruptures is fully proved by two cases recorded in the work of Sir A. Cooper, and the successful termination of both instances proved the judgment and sagacity which had suggested that peculiar treatment."

From the concluding remark of the foregoing paragraph it will be perceived, that great credit is awarded Sir A. Cooper for his success in operating without dividing the peritoneal sack.

Too much importance is no doubt attributed to wounding the peritoneum, and this opinion is well established by the recorded cases in Churchill, of Cæsarean operations, in which mothers were saved. We must look to the anatomy of the umbilical ring for the true explanation of the fatality attending operations for exomphalos: and I think we will learn, that to be more successful, we should apply our different means for reduction in quick succession, so that failing in all but the knife, we may resort to it the sooner. If we introduce a finger into the umbilical ring, through which a protrusion which is reducible has taken place, we will at once be convinced from the firmness and unyielding nature of its walls, that a strangulation occurring at this point, would necessarily result in a disorganization of the tissues much more rapidly, than when it takes place at the internal inguinal ring, which is the mouth of a sac (the tunica vaginalis communis) continuous with the fascia transversalis, a thin elastic membrane, capable of considerable distension under any circumstances. Lawrence relates a case of a lady forty-eight years of age, terminating fatally in seventeen hours from the commencement of the strangulation.

Different methods have been given for the division of the stricture—such as dividing the linea alba downwards, to the extent of an inch; its division upwards has also been recommended, and Velpeau recommends, that several slight nicks be made at different points of the

ring. Dividing the ring downwards to the extent of an inch, would make so large an opening as to present an obstacle in the way of a radical cure. In the case related in the first part of this paper, the ring was divided about a quarter of an inch at the lower part, and this division not being sufficient, a mere nick at the upper part relieved the stricture. In cases where much cutting would be necessary to relieve the stricture, the plan proposed by Velpeau, enables us to relieve the stricture, without augmenting much the size of the ring, as effectually, as by a single long incision in any direction.

The only cases that I have been enabled to find recorded, in which this operation has been successfully performed, are two cases by Sir A. Cooper, one by Mr. Lawrence, one by Mr. Walker of Hurtsperpont, one by Velpeau. All the operations mentioned, were upon females. I have not been able to learn, whether the successful operation mentioned before, by Dr. Wistar, was upon a male or female.—*New Orleans Med. and Surg. Journ.*

Health of the Army on the Rio Grande.—We learn from Dr. Craig, chief Medical Director, who has been at Monterey ever since Gen. Taylor marched upon that place, that the general health of the army has greatly improved since cool weather set in; though many are still suffering from intermittent fever dysentery and diarrhœa. Dr. Craig says that the surgical operations performed at Monterey, for the most part, did very well. He performed one operation at the shoulder joint, on the field, which recovered. We should be much pleased if the surgeons on duty would keep us regularly advised of the health of the army.—*Ibid.*

Prodigious Fæcal Accumulations in the Rectum. By S. A. COOK, M. D.—On the 17th July, 1845, I was called to see F. C., a young lady aged about 15 years. Was informed that she had several times menstruated imperfectly, that she was somewhat troubled with costiveness, and that she had had no evacuation from the bowels during the three days last past. Tongue coated, white, not dry, skin hot and dry, pulse somewhat too frequent, and complained of pain in her head, with perfect loss of appetite. Prescription.—Directed her to take six grains of the following pill mass every six hours. R. Soc. aloes ʒiij.; g. scammony, ʒiss.; pulv. jalap, ʒiij. 1-4; hyd. protochlo., ʒi.; sapo. cast., gr. xv.; nit. pot., ʒss.; tart. ant. ʒj.; oil. anise. arab. muc., aa q. s. to make a mass.

July 18.—Being about ten miles distant, I received a very urgent call to visit her; found her in great pain, like the last pains of labour, the intermissions being very short, yet very perfect; urgent and painful desire to pass urine, yet none had passed since the morning before, (now 4 o'clock, P. M.) Cathartic pills have not operated, and was now informed that all the evacuations during the past two weeks had been but an occasional scanty discharge of mucus, and that such discharges were now being produced, the consequence of

the excessive tenesmus. Deciding to introduce a catheter I attempted to pass a finger into the vagina, but was prevented by what appeared to be an unyielding mass, filling the whole pelvis, and pressing upward and forward so as to make it very difficult to pass the finger between it and the pubes. I accordingly carefully insinuated the point of a silver catheter into the urethra and passed it into the bladder, and discharged a quart or more of urine. The tenesmus still continued, and the acuteness of the pain was somewhat relieved, but the involuntary straining effort which characterizes the closing throes of labor still continued. With considerable difficulty I now passed a large-sized gum elastic catheter into the rectum, and through a mass of fæcal matter, some ten inches, when adapting a syringe to the external end of the tube, I succeeded by dint of perseverance in forcing warm water through the plugged orifice of the upper end. After sending up about a quart of fluid, the catheter was withdrawn, and in two or three minutes more than a gallon of fæcal matter followed, consisting almost entirely of the seeds of raspberries. After another small evacuation, which followed in a few minutes, she became entirely comfortable. The next day I was again called, and finding much the same symptoms, resorted to the same means, and obtained a similar result. After this the urinary bladder and the rectum evacuated themselves without aid, and raspberry seeds continued to appear in the fæces for several days longer, though none had been eaten during the week previous to my first calling upon her. Since that time she has enjoyed her usual health.

I present this case to the notice of the profession, not on account of any peculiarity of the practice; indeed I think it was but what was indicated, and would have readily suggested itself to any reflecting physician; but 1st, To show that a vast amount of fæcal matter may accumulate in the rectum, and also above the sigmoid flexure of the colon, while the sensibility of the mucous membrane remains low as in cases of constipation, but that when this sensibility is increased, as it was in this case by the cathartic, violent symptoms are the consequence; and 2d, That when the pelvis becomes sufficiently full to distend the perineum, the action of those muscles associated in the function of expelling the contents of the pelvic viscera is excited, and if this distension be proportionally increased their action becomes intermittent and involuntary. This phenomenon we have all so frequently witnessed in parturition, when the head of the child fully occupies the pelvis and rests on the perineum, that we find it difficult to view it as but a specific accompaniment of that series of phenomena, the aggregate of which constitutes labour. Indeed so strong did this influence operate upon my mind, in this case, that when preparing to introduce the catheter, notwithstanding the youth of my patient, and the character of the family being above suspicion, I could not divest myself of the feeling that, upon the finger entering the vagina, the head of a fœtus would present itself.

Buskirk's Bridge, N. Y., Jan. 4th, 1847.

Boston Med. and Sur. Jour.

Letter from Persia.—Leeches in Intermittent Fever, &c.

We have just heard that the cholera has broken out in Teheran, the capital, though we have no definite information yet of the extent of its ravages. A physician residing in Tabreez, writes me, under date of August 1st, in the following manner: "We are all consternation here, at present, at the news of the cholera being at Teheran. The Prince (Governor of this part of Persia,) is most anxious to take all sanitary measures to prevent its arrival here, and has had communication with Dr. C. and myself on the subject, since which all the Rabab manufactories (cook-shops) have been sent outside of the town, and the selling of fruit diminished. Cleanliness is strictly enjoined, and a fresh supply of water let into the town from the gardens for watering the streets." The disease marched through these parts some twelve or fifteen years ago, and made dreadful havoc of human life. In many cases the natives, when attacked by it, used to throw themselves into a fountain or stream of cold water. The testimony is, as you would expect in such cases, that some lived and some died. By the way, what do you think of Andral's summary of the cholera? "Anatomical characters, insufficient; causes, mysterious; nature, hypothetical; symptoms, characteristic; diagnosis, easy; treatment, doubtful."

Are you familiar with the use of leeches, applied over the region of the spleen, in obstinate cases of intermittent fever? I have tried this treatment in two very bad cases, and with the most complete success. A Roman Catholic priest, a native of France, had been my patient some time, and I had become almost discouraged in using quinine and arsenic. He became worse and worse, and, dropsical symptoms supervening, his friends thought he would certainly die, and I began to think so too. In this state, I heard that Dr. Bell, Physician to the British Embassy at the Court of Persia, had used leeches in similar cases with great success, and I determined to try them on my patient. Dr. Bell's mode is to apply them on the day the moon fulls, and to repeat them every full moon until the disease is conquered. Though I had, of course, little confidence that the moon had any thing to do in the matter, I made the experiment exact to the letter, and at the first full moon applied twelve leeches over the spleen, intending to repeat the application at the next full moon in case the disease did not give way. But to my delight, and, I may add, surprise too, the poor priest, sallow and dropsical as he was, began to recover from that day, and I had no occasion to repeat the leeches.

I recently made a professional visit to Badr Rhem Bey, the celebrated Roordish chief of Buhtem on the river Tigris. His son, a youth of 12 or 14 years of age, had been suffering for about a year and a half from ague and fever of the *quartan* kind. He was reduced to a state which gave much uneasiness to his friends. At first I tried a purge and quinine, but without success. Indeed his next attack was more severe than before he had taken the medicine. I concluded to resort to the use of leeches, though some of his friends

thought he would certainly die, if he were to lose blood, pale and emaciated as he was. Without regarding the moon as in the former case, I applied six leeches the day before he expected a paroxysm, and on the morning of the next day, gave him two doses of quinine of four grains each. The disease was broken from that time. What comments have you to make on these cases?

Many, many thanks for your Journal. It is always *very* acceptable.

I remain, my dear sir, yours very truly,

A. H. WRIGHT.

Oroomiah, August 12, 1846.

Ibid.

Royal College of Surgeons of England.—We cannot afford space for these regulations in full. They may be had at the booksellers, or in our number of last year, of which copies may be obtained at the office. From them it appears that a candidate for the *fellowship* must be twenty-five years of age; that he must have “a competent knowledge” of Greek, Latin, French, and the elements of mathematics. It does not, however, appear that what is called a “competent knowledge” of those matters is defined. He must have been engaged in professional study in schools or hospitals for six years, three of which, at least, must have been passed in *London*. He must have attended a surgical hospital for four years, and a medical one for one, and lectures on anatomy with dissections for three winter sessions; also, lectures on the practice of medicine, and clinical medicine, surgery, and clinical surgery, for two sessions, and on chemistry, materia medica, midwifery, medical jurisprudence, and comparative anatomy, for one session. He must also have served as house-surgeon or dresser in a recognized hospital. Bachelors of arts of “*English*” universities are admitted after five years’ study, and are not examined in classics or mathematics. Those who were *members* of the college in September, 1844, are admitted to examination for fellowship when they are of eight years’ standing, and those since admitted, or to be admitted members, are admitted to the same examination after twelve years’ standing; but the latter must have graduated in an *English* university, or produce a certificate of “competent” classical knowledge. Candidates for the fellowship are examined on “anatomy, physiology, pathology, therapeutics, and surgery,” only; and are required to perform dissections and operations on the dead body. It does not appear that they are examined on chemistry, the practice of medicine, materia medica, medical jurisprudence, comparative anatomy, or midwifery. They are, in fact, to be pure surgeons, and not general practitioners, and cannot claim to be appointed to mixed medical institutions, such as the Irish Infirmaries and dispensaries. They are to be what is called in England “pure” surgeons.

Candidates for the *membership* of the College of Surgeons of England are required to produce a certificate of being twenty-nine years of age, and having been engaged “in the acquirement of professional knowledge” for four years, and of having studied practical pharmacy for six months. The meaning of “acquirement of professional

knowledge" and "study of practical pharmacy" is not defined. Irish pupils going to London are said to take with them "a certificate" to that effect from any apothecary, which is procured for a trifling *douceur*. Candidates for the membership are required to attend an hospital for three years, nine months in each year. All the Dublin hospitals are recognized. Candidates are also required to attend lectures on anatomy and physiology, and demonstrations with dissections for three winter sessions, surgery for two, and the practice of physic, chemistry, materia medica, and midwifery, for one. Candidates for the membership are examined in *anatomy and surgery only*. In England the qualification to practice medicine and pharmacy is derived from the Apothecaries' Company, and no man is considered a general practitioner there, or eligible as medical attendant to mixed medical charities, unless he holds their license. The College of Surgeons in England, therefore, *does not examine in practice of medicine or pharmacy, or pretend to give any authority to practice either*. Neither do they require their members to learn or to answer in medical jurisprudence, considering that medical witnesses should have the full qualification of general practitioner. In fact, it appears that the college wish it to be understood that their members are qualified as mere or "pure" surgeons only, and leave them to obtain their qualifications to practice medicine from the medical colleges, and pharmacy at the apothecaries' halls.

With respect to certificates, the London College recognizes all schools and lectures without exception. They state, however, in the above regulations, that, in common with all the other colleges, they reject the certificates of persons who lecture on more than one branch, and therefore refuse the certificates of professors or lecturers who lecture on surgery as well as on anatomy and physiology, but they allow this rule to be evaded. Certificates are not recognized from Dublin, unless the name of the candidate who produces them shall have been returned to the college as having been in attendance on the 25th of November, the 10th of February, and the 10th of May; at which periods the professors, lecturers, and hospital surgeons, are required to return the names of the students then attending, *with the dates of entry*, and periods for which they have entered, "with remarks." "It is required that the dates of commencement and termination be clearly expressed." Students should henceforth be very careful not to rest satisfied with having their names entered into these returns, unless they are really in Dublin and actually "attending" the lectures and hospitals, because any falsehoods in the returns may subject them to the penal provisions of the charter. It is the person who holds, uses, or presents false certificates, who is to be punished, the person who signs and grants them is not held answerable.

There are various methods by which students whose other avocations or limited finances prevent them from complying with the printed regulations of this college can obtain an examination; and as the council appears disposed to afford gentlemen so circumstanced all reasonable facilities towards obtaining a diploma, they can scarcely

be blamed for availing themselves of them. An experienced private examiner can explain more to them respecting this matter than we care to mention.—*Dublin Medical Press.*

Progress of the Cholera.—We copy the following from the *Times* newspaper, and we do so at the risk of exciting perhaps unnecessary alarm. It is obvious that the medical profession should not be kept in the dark with respect to such a matter, if for no other reason, for this, that they should be prepared, not only to meet real danger, but to resist and expose any attempt to create premature or uncalled for apprehensions for placehunting or puffing purposes.

“We have received from our correspondent at Trebizonde a letter dated the 26th of September, from which, with deep regret, we make the following extract:—

“The cholera has passed the line of the Russian quarantine on the borders of the Caspian Sea, and is raging throughout all the Tartar villages of the district of Salgau and of Leukeran. A considerable number of Cossacks, from the *cordon* on the Persian frontier, have likewise been attacked. At Rescht, a Persian city in the province of Ghilan, the cholera is still making incessant ravages, which have now continued during two months. The sanitary state of all the towns to the west of the Caspian Sea, from Bakou to Astrachan, is very unfavorable. Dysenteries and diseases of the stomach (frequently mortal) prevail in those towns, particularly amongst the troops in the garrisons. These maladies are probably the forerunners of the real Asiatic cholera—a phenomenon rather curious, which has been again observed latterly in Persia. There prevailed at Teheran, at Astrabad, at Meschid, and at Ispahan, a malady a considerable time before the appearance of the cholera, of which the symptoms resembled the Indian disease. The caravans which arrived from Teheran eight months since spoke of the existence of the scourge which was mistaken for the cholera. A French physician, who resided at Teheran at that period, and who passed through our city a few days since, assured us it was the cholérine, such as had likewise been observed in several towns of Europe in 1833 and 1834* as a precursor of the cholera. The population of Teheran, which had been estimated at 80,000 is reduced by the ravages of the cholera to 60,000. The foreign ministers and their attendants had not dared to return to the city, but still continued to reside at Mount Albus, in the neighborhood of Schemen, to the north of Teheran. The Russian authorities at Tiflis are well aware of the appearance of the cholera in that neighborhood, and the inhabitants of Tiflis have fled, but up to the 12th of September no official announcement had been made of the fact. Perhaps this course was pursued in order to prevent the merchants from becoming alarmed, and a consequent interruption of commercial affairs.”—*Ibid.*

* This is a mistake. The cholera set in in Paris on the 28th of March, 1832.

Free trade in Medicine and Surgery.—The people called “statesmen” as a grove is called *lucus, a non lucendo*, have taken into their wise noddles the strange crotchet that every man, woman, and child in England, inherits and enjoys the unquestionable right to cripple, poison, mutilate, and slay all and every her Majesty’s subjects, provided they consider that they can thereby put money in their purses, and all dyspeptic lawyers and constipated jurymen have evidently arrived at the same conclusion. Mr. Warburton, the great theoretical medical reformer, by his advocacy of cheap-doctoring and free trade in drugs, did much to perpetuate this delusion, and Sir James Graham by his abortive bill contributed to the same end. Every day’s experience, however, proves that there must be an end to this nonsense. The recorded deaths from the mal-administration of drugs form a frightful catalogue of wanton outrages within the present year, and the evil every day increases. The following is another example of the toleration extended to the base and ignorant who have the temerity to trifle with life in this way. Judges, lawyers, and jurors have become so familiarized with such displays that they have now become subjects of jest, or occasions for cracking jokes on the doctors.—*Dublin Med. Press.*

“A report having been circulated that a man named William Myhill, a small farmer and carpenter, residing at Horsey, in the county of Norfolk, had died from the effects of some medicine which had been administered to him by his wife, Mr. Pilgrim, the county coroner, directed the body to be exhumed, and on the 24th of last month held an inquest at Catfield, where the body had been interred. Several witnesses were examined, but the chief evidence offered was that of the servantmaid, who, in a long statement, disposed to her mistress having obtained some medicine of a person living at Reepham, which she administered to the deceased just previous to his death, and then requested her (the servant,) not to say anything about it to any person, but to deny it if she was asked any questions on the subject. On Friday, October 2nd, the inquiry was resumed, when amongst other witnesses who were examined as to the wife having administered something to the deceased, was a Mr. Staples of Reepham, who calls himself a chemist and druggist. He deposed as follows:—

“I vend drugs and prepare them, but I do not profess to be a surgeon. Some short time since Mrs. Myhill, the wife of the deceased, came to me and stated that her husband was very bad. I prescribed for the deceased from the representation made to me by his wife. I cannot say what she stated. I made up some medicine according to the nature of the disease. I was not told what was the matter with him, but I found it out by my study, my science, and my search. I do not recollect that I ordered brandy and water, neither do I exactly recollect what I did prescribe. At the time I put it on a slate, but it was afterwards rubbed off. The medicine was to relieve the pain—it was not opening medicine. Mrs. Myhill was to have called upon me again, and let me know how her husband was, and to tell

me the effect the medicine had upon him. I am perfectly satisfied that the medicine I prescribed could not do him any harm, but I did intend that it should do him good. I considered that the deceased was in a very bad state, and that I ascertained from my research in science, and study from my books of knowledge. If a person came to me and represented their case, I should not be governed by what he said, but I should be governed by the rule of science and my books of knowledge. I could by searching those books ascertain more of their disease than any person could inform me. It is a very common practice with me to prescribe for persons I have never seen, nor yet had a description of their complaints. I neither want to know the name of the party, or where they come from, or any description whatever of their complaints, as I can always find every thing out by the rule of science, my study, and from my books of knowledge. If any person had come to me after the death of Myhill, I could have stated the cause of his death, but the time is now so far gone that I cannot. He again repeated his powers of discovering the complaints of persons by the aid of his books, which was the cause of much merriment to the coroner and the jury, who looked with some suspicion upon the many *cases* [cures?] he pretended to have effected by his books, his science, and his study.

After this evidence, which put a very different aspect upon the inquiry, the surgeons, who had analysed the stomach, said that they had not been able to detect the presence of any metallic or vegetable poison; and, from the appearance of the lungs, were of opinion that the deceased died from natural causes. The jury returned a verdict accordingly."—*Norwich Mercury*.

A New and Original Curiosity of Medical Literature.—Wonders will never cease. Who could have thought some time ago that a "celebrated obstetric physician" in the metropolis could treat a lady in the country by electrical telegraph, yet to that complexion has it come.—*Ibid*.

"Consultation per Telegraph.—The services of the electric telegraph between Norwich and Shoreditch were put into requisition on Thursday in a novel manner, being made the means of communication between a physician in London and his patient in the former place. On Wednesday Dr. L., a *celebrated obstetric physician*, was sent for from London to attend a lady, lying there in a dangerous state; on his return to town, he left instructions to the medical attendant to convey information of the state of the patient the next morning by means of the telegraph. This was promptly done on Thursday morning, and the prescription was as promptly returned. This it would appear, was repeated more than once, the services of the telegraph being continued for four hours. Unhappily the telegraph completed its communication by announcing the *death* of the lady." *Essex Herald*.

The Asiatic Cholera in Persia.—According to the Gazette Médicale, six Princes and several Princesses of the Court of Persia have been cut off by the Asiatic cholera. The mother of the Prince Royal, and the only daughter of the Schah, had been attacked, but had recovered under the treatment of Dr. Cloquet. Among the victims is the celebrated Mirza-Aboul-Assan-Khan, minister of foreign affairs, —who was ambassador to this country in the year 1820. Another minister of the Schah, the Visier of the Prince Royal, and other high functionaries of the Court, have also been cut off by cholera. The disease appears to have been particularly fatal among the upper classes. It was spreading in all directions, and had taken the course of Astrachan and Moscow. It was expected, however, that its progress would be arrested by the cold of winter.—*London Med. Gaz.*

On the Influence of Vaccination in Diminishing the Mortality from Small-Pox.—Much has recently been said respecting the frequency and fatality of small-pox, at the same time that doubts were often expressed as to the efficacy of vaccination in preventing that virulent disease. Should any person still entertain such opinions, his fears must be very considerably diminished by the facts stated at page 62 in the Seventh Report of the Registrar-General, just published. According to that official document, the number of deaths from small-pox, throughout England and Wales, during five consecutive years, were as follows:—In 1838, 16,268 persons died of that disease; in 1839, 9,131; in 1840, 10,434; in 1841, 6,368; and in 1842, only 2,715 deaths are reported.—*Ibid.*

Employment of Gun-Cotton in Cupping.—The Prov. Med. and Surg. Journal of Dec. 9th contains the following announcement:—“It may be useful to know the value of gun-cotton in exhausting the air from cupping glasses; having so employed it myself on several occasions, I can recommend it as possessing a decided superiority over spirit; besides, its lightness and portability is an advantage at times. A very small portion is placed within the glass, and before a piece of lighted paper can be well introduced, from its highly inflammable nature it becomes ignited, imparting to the surface enclosed merely an agreeable warmth.”